A JOURNAL OF NATURAL PHILOSOPHY, *CHEMISTRY*, AND THE ARTS.

VOL. XXVIII.

Illustrated with Engravings.

BY WILLIAM NICHOLSON.

LONDON:
PRINTED BY W. STRATFORD, CROWN COURT, TEMPLE BAR; FOR
W. NICHOLSON,
No. 13, BLOOMSBURY SQUARE;
AND SOLD BY
J. STRATFORD, No. 112, HOLBORN HILL.

1811.

PREFACE.

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The Engravings consist of 1. A Machine for producing small Quantities of Electricity, by J. A. De Luc, Esq. F. R. S. 2. Delineations and Dissections of various luminous Animals, from James Macartney, Esq. 3. Diagrams for the Calculation of the Height of Meteors, by N. Bowditch, Esq. 4. Joint of the Phaseolus Coccinea, delineated from Nature, by Mrs. Ibbetson. 5. Changes in the Stamens and Pistils of the Passion Flower, by Mr. R. Lyall. 6. Influence of Galvanism in Metallic Arborizations, by C. J. T. de Grotthuss. 7. Diagrams to illustrate the Motion of Rockets, by W. Moore, Esq. 8. Dissection of the Petals of various Flowers, showing the Structure from which the Beauty of their Colour arises, by Mrs. A. Ibbetson. 9. Parts of the Corollas of different Flowers delineated from Nature, by the same Lady. 10. Two new Constructions of the Air Pump. 11. Dissections of Plants, showing the Growth of the Bud, by Mrs. A. Ibbetson. 12. Dissections of Plants to illustrate the Growth of the Bud, by Mrs. Agnes Ibbetson. 13. A Machine for Evaporating Liquids without Heat, by the late Mr. Joseph Montgolfier.

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JANUARY, 1811.

ARTICLE I.

Experiments concerning the ELECTRIC MACHINE; *showing the Electric Effects of* FRICTION *between Bodies. By* J. A. DE LUC, *Esq. F. R. S.*

THE invention of the *electric machine*, which has been within my time, and of which I have followed the progress, was, with respect to the long known effects of *friction* on *amber* and other substances, what Sig. Volta's *pile* has been to the discovery made by Sig. Galvani of the *motion* produced in the limbs of a recently dead frog by the association of two proper *metals*: I mean, that, by increasing these effects, a particular *fluid* has been found to be the cause of both. However, though this *fluid* has been so long discovered by the *electric machine*, its nature has not yet been agreed upon among all experimental philosophers. Invention of the electric machine. History and progress of the invention.

A rapid motion communicated to small globes of different substances, rubbed by the hands, was found to produce certain effects susceptible of being propagated at an unlimited distance through metallic wires, suspended by silk, or supported by glass; and these effects consisted, not only in greater *motions* of bodies than were produced by the *friction* of *amber*, but in strong *luminous* phenomena.

Two kinds of electricity.

The greatest effect of *friction* before observed having been produced by *resinous bodies*, globes were made of them, especially of *sulphur*, for electric machines, at the same time as globes of *glass*, and they both produced the *divergence* of pairs of *balls*; but it was soon observed, that the *divergence* produced by one kind of globe was destroyed by the other kind: whence arose the system of two kinds of *electricity*, which were supposed to *neutralize* each other when *united*, and to be manifested only when separated, producing then their peculiar effects; one of them was called *vitreous* and the other *resinous*.

This new agent,

This new object of study engrossed the attention of all natural philosophers, not only on its own account, but because of the deficiency of known *agents* in a great number of natural phenomena; and this interest having been increased, when the analogy between *lightning* and some of the effects produced by the *electric machine* was discovered, no wonder that many hasty systems were formed for applying this new, but undetermined *agent*, to various phenomena not yet satisfactorily explained, even to the motions of celestial bodies.

Franklin's theory,

The idea of two different *electricities* continued predominant among natural philosophers, till Dr. Franklin explained the difference between the electric phenomena attributed to this double cause by only *more* or *less* (*plus* or *minus*) of the same fluid: but as this ingenious experimental philosopher assigned the intermediate state between *plus* and *minus* to a certain *quantity* of *electric fluid* belonging to bodies, his theory, though adopted by the majority of electricians, remained subject to insuperable objections under this form; which objections nobody more forcibly urged than Dr. Peart, to whom no solid answer has been made, nor could be made, as long as the theory remained on the same foundation. Thus many experimental philosophers have retained the idea of two distinct *fluids*, as producing these two effects which destroy each other: but different hypotheses have been framed on the nature of these fluids, which have been even transformed into certain properties belonging to bodies. However the duration of this variety of systems proceeds only from a want of attention to the complete

exposed to strong objections,

till removed by Volta.

complete solution of all the difficulties, which occurred in the system of *plus* and *minus*, given by Sig. Volta, in fixing the real point to which these *positive* and *negative* quantities are to be referred, namely, the *actual*, but different at different times, *electric state* of the *ambient air*.

Only one electric fluid.

According therefore to this system, all the *electric* phenomena are produced by one *fluid*, the modifications of which in terrestrial phenomena deserve the most attentive and assiduous study of all experimental philosophers: but no agreement among them, either on the importance of this study, or on the mode of its pursuit, can be expected, as long as they shall be so far divided in their opinions on the nature of this agent, as to leave a doubt whether it consists of one and the same, or two distinct *fluids*, or even of some properties of bodies.

Experiments on the effects of friction.

Friction between bodies being the only method, hitherto known, by which can be produced at will the two kinds of *electric signs*, I thought that the most direct and sure method of coming to a better knowledge of their meaning was to study them at the origin of the two distinct phenomena; and I undertook a series of experiments on the effects of *friction* between various kinds of bodies; *insulating* those which are more or less *conductors*, and applying *electroscopes* to both the bodies which exercised *friction* on each other; in order to observe the modifications thus produced, not only on the *nonconducting*, but on the *conducting* bodies. I made these experiments with large apparatusses, by which the reciprocal effects of *friction* between various bodies were manifested by the motions of pairs of balls; and from them I derived, without exception, the theory which I shall first explain, and then prove by an account of some of the principal experiments.

Theory. Conductors and nonconductors.

I. The *electric fluid* resides on all terrestrial bodies, every particle of *air* included; being retained upon them by a mutual attraction, which however differs in degree, according to the bodies: some attract the *electric fluid* only when it comes into contact with them, but then it adheres strongly to the parts which receive it, moving very slowly along the surface of these bodies, which therefore are *nonconductors*: others receive it at more or less distance, and it is propa-

 gated

gated more or less rapidly along their surface. *Glass*, though absolutely *impenetrable* to the *electric matter* (as I have explained in the preceding paper) permits the *electric fluid* (i. e. *electric matter* united with *vector*) to move with a sensible progress along its surface.

Effects of friction.

2. Friction excited between two bodies has no other effect than that of disturbing the natural *equilibrium* of the *electric fluid*, which tends always to be produced among all bodies according to its *actual* (but local, in a certain extent) quantity on them and in the *ambient air*. If both the bodies, which exercise *friction* on each other, are good *conductors*, this disturbance, the equilibrium being constantly restored, is not perceived; but if one has more disposition than the other to attract the *electric fluid* thus agitated, with the faculty of transmitting it to its remote parts; when they are separated, either suddenly, or in general before the *equilibrium* of the fluid can be restored between them, one is found *positive*, as having acquired a proportional quantity of *electric fluid* greater than the *ambient air*, and the other *negative*, as having lost that quantity; both being supposed to have previously possessed the same electric state as the *ambient air*.

3. The general effect therefore of *friction* between two bodies is, that one never becomes *positive*, without the other being made *negative* (or vice versa). This evident proof, that all the *electric phenomena* which we are hitherto able to produce at will, namely by *friction*, proceed from the disturbance of the *equilibrium* of only *one fluid*, will be afforded by the experiments, which I shall here relate after another remark.

Negative and positive not properties of different bodies, but different states of the same body.

Mr. Cavallo, in his *Complete Treatise on Electricity*, (3d edit. vol. i, p. 21.) has given a table containing the results of his experiments of this kind, wherein is found, that certain bodies become either *positive* or *negative*, according to those by which they are *rubbed*. This circumstance had already shown, that *negative* and *positive* were not properties belonging to certain bodies, but only different states produced on the *same* body by different circumstances, and in particular by the difference of those which exercise friction on it: however, there remained to be known what effect, in the latter

latter case, was produced on each of the bodies which exercised that *friction*. This had been one of my objects in the above mentioned experiments; for which purpose I kept *insulated* both bodies exercising *friction* on each other, applying to them *electroscopes*. It is by experiments thus performed, that I found the general law above expressed; for when any of the bodies, which may be rendered either *positive* or *negative* by *friction*, was brought to one of these states, the body which had exercised it was constantly found in the opposite state: if the former became *positive*, it had *taken* some *electric fluid* from, and if *negative*, it had *yielded* some *fluid* to, the body which had exercised *friction* upon it. Such are the effects, which will be seen in the following experiments.

Apparatus contrived to prove the similarity between the electric machine and pile.

The small apparatus, which I am going to describe, was not at first intended for this purpose; my view in its construction having been to prove the kind of analogy which I have established in my former paper, between the *electric machine* and the *electric column*, and even the *pile*, with respect to its *electric* property; the only difference in this respect between the electric machine and the latter instruments being, that, as in the former the effect is produced by *friction*, it requires a mechanical process for its continuance; while in the *column* and in the *pile* it is produced by only the association of two proper *metals*. The surest mode of following the course of the *electric fluid*, when its equilibrium is disturbed, is by the motions of *electroscopes*; but the immediate comparison which I had in view could not be made with any of the usual *electric machines*; as the *gold-leaf* being the only *electroscope*, that can be effected by a *pile* or *column* of a moderate size, it would be torn to pieces by the smallest *electric* machine hitherto used. This made me think of the construction of so small an *electric machine*, that the *gold-leaf* electroscope should not be more affected by it than by my largest *column*; but after the first experiments I was induced to construct this small apparatus so as to be applied to experiments concerning the different effects of *friction* between different bodies.

This apparatus described.

The dimensions of this instrument, as represented in the figure Plate I, are, in the parts supposed the nearest to the

Apparatus described.

the spectator, half those of the original; the other parts diminishing as required by perspective. A wooden base 1, 1, on which the different parts of the apparatus are fixed, must, during the experiments, be fastened to a table, by proper vices, in order to make it steady while the instrument is worked. On this base are solidly fixed two wooden pillars, 2 and 3, the former of which is bored a little way in, at the top 4, in a horizontal direction, for receiving one end of different *spindles*, as 5, the other end of which is lodged in a groove at the top of the pillar 3, and projects outwards to receive a winch 6, by which all the *spindles* are made to revolve: they are prevented from moving upwards in the groove by a piece of wood 7, placed when they have been laid in, and outwards, by a ridge on the *spindle*, bearing against the pillar 3 (as 5, fig. B.). The *prime-conductor* 8, is made of a *brass* tube stopped at each end; it is supported by a *glass* pillar 9, covered with insulating varnish, and fixed, on the base 1, 1, by its own wooden base 10. One end of this *conductor* communicates with the *spindles* of whatever kind, by means of a very thin slip of whale-bone covered with gilt paper, fixed to a brass piece, which screws into the end of this *conductor*: this screw has some length, in order to move it backwards or forwards, so that the extremity 12 of the whale-bone slip may attain the proper point of the *spindle*: the slip, which is elastic, presses against the *spindle* by a natural bent upwards. This part of the apparatus is the most difficult to adjust; the proper bearing of the extremity 12 of the slip, on the different *spindles*, must be found by trials; in general, it must bear, beneath the *spindles*, at a small distance from the extremity 16 of the various *rubbers*. The other end of the *prime-conductor* communicates with the *gold-leaf* electroscope 11.

There are different kinds of *rubbers*, but they are all fixed in the same manner to the apparatus: they consist, either wholly, or in great part, of a very elastic *brass* lamina, and differ only by that end of them, 16, which presses on the *spindle*: the other end enters laterally, and is fixed by a wedge, into a notch horizontally cut in the part 15 of a brass piece 13, held on the top of a glass pillar, 14, covered

covered with an insulating varnish, and solidly fixed into the wooden base, 1, 1. The different kinds of *rubbers* must press with different degrees of force, according to their nature, and yield always easily to the inequalities of the *spindles.* For these purposes, a part of the pressure is exercised by one or two additional brass springs, as at 17, pressing on the main spring 16. The *brass* piece 13, by the wire 18, serves as *conductor* from the different *rubbers* to the *gold-leaf* electroscope 19. Another wire, fixed to the same piece at the point 20, projects from it, and is terminated by a hook 21: this hook is to receive, occasionally, the hook 22, of a wire belonging to a *glass tube* filled with water 23, the other wire of which is hooked to the *prime conductor* at the point 24. The hook 22 is commonly held up by a silk thread, passing over an insulated pulley 25, and stopped at the bottom by a bead fixed to the thread, in a notch of the brass piece 26: the bead is disengaged when the hook 22 is to come down into the hook 21.

Such is the general description of the apparatus, and I come to the experiments; beginning this account by those which I had principally in view in its construction, namely, to have an *electric machine* with a power not exceeding that of my *electric column* of 600 groups, for the purpose of comparing their correspondent effects. I made many trials before I could bring an *electric machine* down to so small a power; the following was that which nearly succeeded, though still too strong.

Difficult to reduce its power to that of the column.

Exp. 1. The *spindle,* 5, is here a solid *glass* cylinder very little above ¼ of an inch diameter; it is covered with *sealing wax* at both ends, for the purpose of insulating a naked part, about ¾ of an inch in length, in the middle of which the *friction* is produced. The *rubber* is a simple *brass* spring, so bent at its extremity, 16, as to fit the upper part of the *glass* cylinder.

Exp. 1.

This apparatus in fact is an *electric machine,* with a *glass cylinder* of only ¼ of an inch diameter; however, small as it is, it must be moved very slowly, or the *gold leaves* would have greater motions in striking the sides, than they have at the extremities of my *column* of 600 groups, even in the time

Shows the small quantity of the electric fluid acting in the pile.

time of its greatest action. This experiment therefore directly shows how small is the quantity of *electric fluid*, which, modified in the *pile*, produces such great *chemical* and *physiological* effects; and the following will prove, that the *motion* of the *electric fluid* is the same in both instruments, though produced by different causes.

Exp. 2.

Exp. 2. While the machine is in motion, if I loosen the silk thread which holds up the hook, 22, of the wire belonging to the *glass tube* with water, 23, in order that it may fall into the hook, 21, and that the communication may be thus established between the *rubber* and the *prime conductor* through the *water* of the *glass tube*; at the instant that the *hooks* come into contact with each other, the very *small* quantity of *electric fluid* there in motion pervading the *water*, the *gold leaves* fall in both electroscopes, and never rise though the machine be kept in motion. This is the same effect as is produced on the *column* in *exp.* 29 of the former paper; for the divergence ceases also in both its electroscopes, when a similar *tube* connects its extremities: in both cases, the *electric fluid*, which tends to accumulate on one side, passes instantly to the other, and thus keeps up the equilibrium. That such is the effect of the *circulation* of only *one* and the same *fluid* set in motion, by *friction* in the *electric machine*, and by the association of the two *metals* in the *column*, will be proved by the effects of *friction*, an instance of which is already seen in the former case.

Proofs that only one fluid circulates.

In the above experiment, *friction* is exercised between two bodies, one of which ranks among the best *conductors*, namely *brass*; however the effect is observed upon it, because it is *insulated*: the other is the *glass* cylinder, which, passing under the *brass* rubber, certainly takes *electric fluid* from it, since, transmitting some to the *prime conductor*, and by this to the electroscope 11, the latter diverges *positively*; and since at the same time the *brass* takes off some *fluid* from the electroscope 19, which thus diverges *negatively*.

Effect of the amalgam on common rubbers.

This experiment shows moreover what is the effect of the *metallic amalgam* laid on the *rubbers* of the usual *electric machines*. The base of those *rubbers* must be a cushion, in

in order to yield to the inequalities of the *cylinders* or *plates* while it presses upon them; but the materials of the cushions are not sufficiently *conducting* to permit the loss, which its rubbed part undergoes, of the *fluid* carried off by the *glass*, to be soon repaired by the *ground*; whereas this is effected by the *metallic amalgam*, from which the *glass* takes readily some *electric fluid*, as, in the above experiment, it takes it from the *brass rubber*. Metallic *plates* would produce the same effect on the *cylinders* and *plates* of *electric machines*, if the former were sufficiently pliable.

General effect of friction

The general effect of *friction* between two bodies, as I have said above, is, that if one becomes *positive* or *negative*, the other is found in a contrary state. This holds not only between bodies of different natures, but even between the same kinds of bodies, if one be made to pass in length over one part only of the other. This effect cannot be observed with perfect *conductors*, as on them the *equilibrium* of the *electric fluid* is instantly restored; but there is a known experiment made with two pieces of the same *silk riband*, in which, by making one piece to pass rapidly in length on one part only of the other, the former becomes *positive*, by carrying off some *electric fluid* from the latter, which thus is rendered *negative*, by losing that fluid. I have tried the same experiment with *glass*, in the following manner.

exemplified with two pieces of riband, and

Exp. 3, with glass.

Exp. 3. At the end of a brass spring I fixed a flat piece of the same *glass* as the small cylinder, and used it as a *rubber*, in the same manner as in the former experiments. Both pieces of *glass* being therefore *insulated*, there was no sensible effect on either of the electroscopes; as the immediate effect and consequence of *friction* between these small bodies, being confined within the bodies themselves, were too small to be perceived. I then held my finger on the brass spring near the flat piece of *glass*, in order to restore to the latter, from the ground, the *fluid* carried off by the *glass* cylinder; and having turned rapidly the winch for a little time, the electroscope 11 diverged *positively*. This is the same effect as produced on the *riband* passing rapidly over a part only of another similar: the *glass*, which turned, carried off some *electric fluid* from the other *glass*, which was rubbed only on one point; and as the latter now re-

paired

paired its losses from my finger, the cylinder, which received more *fluid*, communicating thus more of it to the prime conductor, that quantity became sensible to the electroscope 11. The *glass* rubber, undergoing these *losses*, was at every instant rendered *negative*, but not in a perceptible manner, because its losses were also instantly repaired from my finger, which, being in the way, prevented the electroscope 11 from showing this state.

Exp. 4. Friction between glass and sealing wax.

Exp. 4. I covered with *sealing wax* the under part of the extremity 16 of a brass rubber, similar to that of Exp. 1, and made the *wax* rub the *glass* cylinder; this produced a little divergence in the electroscope 11, which was *positive*, but none in the electroscope 19. This at first appears contrary to the above law, namely, that one of the bodies, which *rub* each other, can acquire some *electric fluid* only by taking it from the other, which must become *negative*. But afterward I considered, that the *sealing wax*, here a mere lamina, covering the *brass* spring, being impermeable to the *electric fluid*, the quantity carried off by the *glass* cylinder from its free surface could not be repaired *through* it by the *brass*, which therefore lost no sensible quantity of the *fluid*. In consequence of this idea, I changed the *rubber* in the manner represented by the separate *fig.* A. A piece of *sealing wax* about $\frac{1}{4}$ of an inch thick, cut underneath so as to fit the *glass* cylinder, is held in its upper part by a doubled piece of thin *brass* (one side of which is seen in 2); and on its top is soldered a *brass* spring 3, the opposite end of which is placed, as usual, in the *brass* piece 13, communicating with electr. 19. By this construction, while the *sealing wax* loses some *electric fluid* to the *glass* cylinder which revolves against it, this loss is repaired by the *fluid* proceeding from the *brass* (as from 2, fig. A), moving *along* the side of the piece of *sealing wax*, which thus is enabled to yield more *fluid* to the *glass* cylinder. Thus the electroscope 11 has a much greater *positive* divergence than before, and the electroscope 19 has now a great *negative* divergence, because the *brass* spring, communicating with it, loses some *electric fluid*, by yielding it to the *sealing wax*.

Exp. 5.

Exp. 5. In a piece of *brass* similar to the above, *fig.* A, instead of *sealing wax*, I placed a piece of *Caoutchouc*, or *indian rubber* of the same shape as the wax. This body produced here a singular phenomenon (observed in other cases) which characterizes the manner in which *friction* acts in these phenomena; its effects changing, according to circumstances foreign to the intrinsic nature of bodies. This experiment is difficult, on account of the tendency of the *indian rubber* to stick to the *glass cylinder*, by which the former is torn or disfigured if the pressure is too great, or the motion too rapid: it is therefore necessary to find the proper degree of both. Now, according to these (or other unperceived circumstances) having repeated this experiment on different days, sometimes the *glass* took off some *electric fluid* from the *indian rubber*, and made the electroscope 11 diverge *positively*; at other times it was the reverse; and in both cases the *indian rubber* was in the state opposite to that of the *glass cylinder*, as shown by the electroscope 19.

Exp. 5. Glass and india rubber.

Exp. 6. It has been seen in *exp.* 1, that a *brass* rubber, applied to the *glass* cylinder, becomes *negative*, the latter becoming *positive*. I covered a similar *glass* cylinder with a thick coating of *sealing wax*, and applied to it a *brass* rubber of the same *breadth* as the former, fitted to the *sealing wax* cylinder: it produced no sensible effect. I suspected, that this *rubber* was too *wide*: because thus embracing, in a great length, half the circumference of the *sealing wax* cylinder, which constantly revolved against it, the *sealing wax* lost and regained alternately *electric fluid* on too great a surface; for the point 12, with which communicates the *prime conductor*, to preserve any sensible change. With this idea, I reduced the *breadth* of the *brass* rubber, in the part bearing on the *sealing wax*, to ⅛ of an inch; placing the point 12 of the *prime conductor*, immediately under and very near the extremity of the *rubber*. Then the electroscope 11, communicating with the rubbed *sealing wax*, diverged *negatively*, and the electroscope 19, communicating with the *brass* rubber, diverged *positively*.

Exp. 6. Brass and sealing wax.

This experiment shows, in the first place, the errour of a distinction between bodies as *electrics* and *anelectrics* (electric

Distinction of electrics per se improper.

(electric *per se*, and electric *by communication*, as they have been also called); considering *friction* as the test between these two classes of bodies, and placing *metals* foremost among the *anelectrics*. But it is seen in the above experiments, that there is no such distinction, since *brass* undergoes, by *friction*, changes in its *electric* state, as well as *resinous* bodies and *glass*: only these changes, in conducting bodies, cannot be manifested without *insulation*; and the want of this condition in the experiments of former electricians was the cause of the errour.

Neither are there two kinds of electricity.

The same experiments show also, that *brass* (as a *metal*) becomes either *positive* or *negative*, according to the body with which it undergoes *friction*. In the above experiment the *brass* rubber was made *positive*, because it took off some *electric fluid* from *sealing-wax*; but in *exper.* 1 it was made *negative*, because it lost some of the *fluid* to *glass*. But this difference in the *electric* state of the same body, by the difference of those with which it undergoes *friction*, extends farther; and the following experiment will show the errour of the distinction between two electricities, in which one is called *resinous* and the other *vitreous*; and in general between two *fluids*, or two *electric* properties in different bodies.

Exper. 7. Sealing wax and india rubber.

Exper. 7. It has been seen in the last experiment, that the *sealing-wax* cylinder was made *negative*, by being *rubbed* with *brass*. To the same cylinder I applied the *indian rubber*; and with the smallest pressure (to prevent adhesion), and a very slow motion of the winch, the gold leaves struck so rapidly in both electroscopes, that, in order to try the kinds of *divergences* produced, I was obliged to stop at one part only of a revolution: then I found constantly, that the *sealing-wax*, which in the above experiment was rendered *negative* by *brass*, now became *positive*; and that the *indian rubber*, which in *exper.* 5 became sometimes *positive* with the *glass* cylinder, was here constantly *negative*. Therefore *sealing-wax* itself either *loses* or *gains* some *electric fluid*, according to the body which exercises *friction* upon it, and the *rubbing* bodies always experience the contrary effect.

I have

I have made various experiments of the same kind, with various bodies, all affording the same general result; but I shall only relate one more, giving another example of a body which becomes either *positive* or *negative* according to the other body with which it undergoes *friction*. This is one of the beads, of the size and colour of cherries, worn by indian women, which, as Dr. Lind informs me, consist of an inspissated vegetable oil: they are not soluble by water, and are very elastic.

Farther proof of a body becoming possitive or negative.

Exper. 8. I mounted one of these *beads* on a *spindle*, as in fig. B. The *bead*, 1, is traversed by a small *glass* axis, 2, 2, covered with *sealing-wax*, to the ends of which are adapted pieces of wood, 3 and 4, on the first of which is seen, at 5, the *ridge* before mentioned, which prevents the *spindles* from coming out when placed between the pillars of the apparatus.

Exp. 8.

1. With a narrow *brass* rubber, the extremity of which is fitted to the form of the *bead*, the slowest motion of the winch renders the bead *negative*, and the *brass* rubber positive.

2. With a *sealing-wax* rubber like that fig. A, but fitted to the form of the *bead*, with very little pressure and motion, the *bead* becomes *positive*, and the *sealing-wax* negative.

Such are the constant phenomena observed at the fountain head of all the *electric effects*, which it is in our power to produce; and they depend on *friction*, respecting which various systems have been fabricated. Now from the whole tenour of these experiments it may be laid down as a fundamental truth in terrestrial physics, that *friction* has no other influence in *electric* phenomena, than that of disturbing the *equilibrium* of the *electric fluid*, in such a manner, that one body, by *acquiring* a certain quantity of it above what it had before, is rendered *positive*, and that the other is found *negative*, as having lost that quantity. It is only when its *equilibrium* is disturbed, that the *electric fluid* is manifested to us: the *electroscope* is our first test of this disturbance; but if the bodies, either *conductors* themselves, or associated with *conductors*, are of a sufficient size, and the electric difference between them has arrived to a certain

Friction only disturbs the equilibrium of the electric fluid;

and it is then alone manifest to us.

a certain degree, it is manifested by a *spark* darting from one to the other, and the *equilibrium* is thus restored.

I cannot but consider the fixation of this point, after many controversies, as being no less conducive to the advance of our knowledge in the maze of *terrestrial causes,* than the determinations concerning *air* and the *cause of heat*; and no experiments and observations appear to me to deserve more the joint endeavours of experimental philosophers, than those concerning the modifications of the *electric fluid,* in the *atmosphere* and in the *ground,* of which, in my former paper, I have shown the great influence in *terrestrial phenomena.*

Ashfield, near *Honiton,*
12th October, 1810.

II.

Comparative Examination of the Mucous Acid formed by the Action of Nitric Acid on Gums and on Sugar of Milk. By A. LAUGIER*.

Lime in the immediate principles of vegetables.

MR. VAUQUELIN, by his experiments on gums arabic and tragacanth, has ascertained the existence of a considerable portion of lime in the immediate principles of vegetables. The reading of his paper suggested to me the following reflexions.

What becomes of this in converting gums into mucous acid?

1st, What becomes of the lime contained in these gums, when they are treated with nitric acid, in order to obtain the mucous acid?

2dly, Does it not combine with the oxalic acid, which is formed almost at the same time as the mucous?

3dly, As the oxalate of lime is more insoluble in water than the mucous acid, does it not fall down with this acid, when the residuum of the process is washed, and occasion a sensible alteration of its properties?

4thly, How may we ascertain the presence of oxalate of lime in the mucous acid obtained from gums, and separate this calcareous salt from the acid, the purity of which it affects?

* Abridged from the Annal. de Chim. vol. LXXII, p. 81.

To

To solve these questions I undertook the following experiments. Experiments to find this.

I digested 60 gram. [926·4 grs.] of gum tragacanth in eight times their weight of pure nitric acid at 36° [sp. grav. 1·333]; heated the mixture, till it was reduced to the consistence of honey; and then added a sufficient quantity of water. Gum tragacanth converted into mucous acid.

This would not dissolve a white, pulverulent substance, which, collected on a filter and dried in the open air, weighed 9·5 gram. [146·7 grs]. This was mucous acid. The liquid containing the soluble portion of the mixture was of a yellow colour. I evaporated it, till the moment I perceived a slight pellicle form on its surface, when I removed it from the fire. As it cooled, it deposited a great quantity of crystals, some in laminæ, others in needles easily perceived to be oxalic acid. To separate this acid from the mucous, I poured on the mixture alcohol at 40° [sp. grav. 0·817], which dissolved the oxalic acid, without acting on the mucous. This, collected on a filter, weighed 2·1 gram. [32·4 grs]. The alcoholic solution, being gently evaporated, yielded a coloured mass, which I dissolved in water to obtain whiter and purer crystals. Oxalic acid also formed,

The mother waters of this second portion of mucous acid and of oxalic acid contained a mixture of oxalic and malic acids, which I separated by means I shall not mention, as they are foreign to my subject. and malic.

The first portion of mucous acid I obtained was very white. When dried it had the clotted appearance of starch. This I employed in my experiments, rejecting the second portion, which did not appear to me so pure. The mucous acid

To find whether there were any oxalate of lime in this mucous acid, I mixed one part of the nitric acid abovementioned with ten of distilled water, and poured it on the mucous acid. I then exposed the whole to a heat of 40° or 50° [104° or 122° F.] for 48 hours, shaking it occasionally, to promote the action of the menstruum. The supernatant liquor being poured off, ammonia immediately threw down from it a white earthy salt in silky filaments, which had all the physical properties of oxalate of lime. contained oxalate of lime.

A second

A second portion of weak nitric acid, left to stand on the residuum the same length of time, yielded a fresh quantity of oxalate of lime on the addition of ammonia.

It required eight portions of diluted nitric acid in succession, completely to free this mucous acid from oxalate of lime. The quantity was each time less however, and the ninth portion showed scarcely any perceptible indication of it.

The eight precipitates together amounted to 2·5 gram. [38·6 grs.]

This proved by examination of the salt.

It was of importance to ascertain, whether this foreign matter, which had the appearance of oxalate of lime, were in reality this salt. I therefore boiled it with a solution of saturated carbonate of potash, and when the mutual decomposition of the two salts appeared to me complete, I collected on a filter the portion that had precipitated. This precipitate, not so white as the former calcareous salt, and in coarser powder, dissolved with great effervescence in nitric acid. Its solution, which was acrid to the taste, was not precipitated by ammonia, but very copiously by oxalate of ammonia.

The supernatant liquid, which contained an excess of carbonate of potash, was supersaturated by acetic acid, evaporated to dryness, and the residuum treated with alcohol, in order to separate the acetate from the oxalate of potash, which is not soluble in this menstruum. The mixture, being heated a few moments, was filtered, when the acetate of potash passed through with the alcohol, and the oxalate remained behind.

The matter insoluble in alcohol was dissolved in distilled water; and a drop of this solution, added to half a spoonful of lime water, threw down a pulverulent precipitate, evidently of oxalate of lime. The same solution yielded by evaporation crystals of oxalate of potash.

The experiments I have described leave no doubt of the nature of the calcareous salt, that affects the purity of the mucous acid obtained from gum tragacanth.

Other gums similar.

The same experiments repeated on gum arabic, and on that called in the shops gum of Bassorah, which does not dissolve in water, afforded me nearly the same results.

I observed,

I observed, that, in proportion as the mucous acid was deprived of the oxalate of lime, that rendered it impure, it acquired a more flocculent appearance.

Alteration in the appearance of the acid.

To ascertain, whether the mucous acid obtained from the sugar of milk, in which it was first discovered by Scheele, were likewise contaminated with oxalate of lime, I boiled a similar quantity of sugar of milk with eight parts of nitric acid of the same strength as before. The first portions of mucous acid formed I separated by decantation, and added to the residuum a fresh quantity of nitric acid. A second portion of mucous acid was deposited, which, added to the first, made up the weight of 12 gram. [185·3 grs.], or a fifth of that of the sugar of milk employed.

Mucous acid prepared from sugar of milk, by way of comparison.

I remarked, that this mucous acid, after washing, when diffused in water had as flocculent an appearance as that from the gum, when it had been deprived of its oxalate of lime by dilute acid. This led me to presume, that the acid was much purer than that from gum; and my conjecture was soon confirmed by nitric acid having no action on it. This acid could not take up from it the smallest quantity of oxalate of lime in a long continued digestion, for ammonia did not produce the slightest cloud in the supernatant liquid.

This apparently pure.

What besides left no doubt of the perfect purity of the mucous acid from sugar of milk was its easy and complete solution in boiling water. This even proves it to be more pure than the mucous acid from gum, after it has been deprived of oxalate of lime by the means mentioned above: for the latter, when boiled with distilled water, leaves an insoluble flocculent matter, amounting to 0·06 of its weight; which dries into a gray, horny, semitransparent substance, much resembling in appearance the mucous matter, that connects together the particles of animal concretions; though, when thrown on burning coals, it does not emit the ammoniacal and fetid smell of animal compounds, and yields by calcination carbonate of lime. The very small quantity I obtained did not allow me to make such experiments, as would furnish a more accurate knowledge of the nature of this substance.

The acid from gum not perfectly pure when freed from oxalate of lime, as it contained another substance.

From the facts that have been mentioned, we may infer,

General conclusions.

1st,

1st, There is a striking difference between the mucous acid obtained by the action of the nitric acid from gums, and from the sugar of milk.

2dly, This difference consists in the former being always contaminated with a mixture of oxalate of lime, proportional to the lime contained in the gum; while the mucous acid from sugar of milk does not exhibit the least trace of this calcareous salt, and seems to be perfectly pure.

3dly, The mucous acid from gum may be brought to a similar state of purity by a very simple process, which consists, 1st, in removing all the oxalate of lime by repeated digestions in very dilute nitric acid; and, 2dly, in boiling it in water, which dissolves it, without dissolving the flocculent matter, that the nitric acid did not take up.

4thly, The mucous acid from gum, when thus freed from substances foreign to its nature, is exactly similar to that from sugar of milk, possesses all the properties that characterise this acid, and may be employed with equal advantage in the most delicate experiments, or such as require this acid to be of the utmost purity.

Mucous acid from gum contains mucite of lime, when prepared with diluted nitric acid.

Since I wrote the above I have ascertained, that under certain circumstances the mucous acid obtained from gum is mixed with mucite of lime, instead of the oxalate I have mentioned. This happens, when the nitric acid employed is not in a concentrated state, but diluted with water, and the process consequently goes on not rapidly, but slowly. The difference of the results is easily understood.

Reason of this.

If a weak acid be employed, the mucous acid is at first produced alone; it precipitates with the lime, with which it forms a salt of little solubility; and it may be separated from the mixture previous to the formation of the oxalic acid, which requires the concentration of the nitric. If on the contrary concentrated nitric acid be used, the formation of the two acids, though taking place in succession, is very near in point of time; and it may readily be supposed, that in this case the oxalic acid, in proportion as it is formed, lays hold of the lime in consequence of its great affinity for this earth.

I shall add one more fact, that has led me to remark a singular property of the mucous acid, which I purpose to examine more fully. When

When a solution of pure mucous acid in boiling water is gently evaporated to dryness, without separating the crystalline precipitate, that forms during the evaporation; we soon find, as soon as the whole of the liquid is wasted, that the crystals grow yellow, then brown, and are converted into a kind of viscid substance, tenacious, undergoing a sort of fusion, and acquiring considerable hardness on cooling. Mucous acid changed by heat.

The mucous acid, that has experienced this change, is much more sour than usual, infinitely more soluble in water, and wholly soluble in alcohol, so that its properties are in part altered. At first I thought, that I had converted the mucous acid either into the malic, or the tartaric; but the experiments I have made to verify this conjecture do not yet appear to me sufficient, to authorise my advancing any opinion respecting the nature of the change, that takes place in the experiment I have described. Its properties in this state.

III.

Chemical Analysis of a Black Sand, from the River Don, in Aberdeenshire; and of a Copper Ore, from Airthrey, in Stirlingshire. By* THOMAS THOMSON, *M. D. Lecturer on Chemistry, Edinburgh* †.

THE specimen, which formed the subject of the first of the following *analyses*, was brought from the banks of the river Don, about seven years ago, by my friend Mr. James Mill, who at that time resided in Aberdeenshire. By him I was informed, that considerable quantities of it are found in different parts of the bed of that river,—that it is called by the inhabitants *iron-sand*,—and that they use it for sanding newly written paper. I tried some experiments in the year 1800, in order to ascertain its nature; but was too little skilled at that time, both in mineralogy and practi- Black sand on the banks of the Don.

* For an account of a black sand, consisting of a ferriferous ore of titanium, found on the shores of Liguria, and traced to the rock from which it proceeded, see our Journal, vol. XXVI, p. 94.

† Trans. of the Royal Soc. of Edinburgh for 1807.

 cal

cal chemistry, to manage an analysis of any considerable difficulty.

mixed with the detritus of granite or gneiss,

The black powder is mixed with a good many small, whitish, reddish, and brownish grains, which, when examined by means of a glass, prove to be pieces of quartz, felspar, and mica. From this it would appear, that the sand of the river Don consists chiefly of the detritus of granite or gneiss.

and consisting of two substances,

When a magnet is passed over the sand, some of the black grains adhere to it, and are by this means easily obtained separate. But after all that can be attracted by the magnet is removed, the greater part of the black powder still remains. This residue is indeed attracted by a powerful magnet, but so very feebly, that it is not possible by means of it to separate it from the grains of sand with which it is mixed. Thus we learn, that the black matter consists of two distinct substances; one of which is powerfully attracted by the magnet, the other not. As this second substance was obviously specifically heavier than the grains of sand with which it was mixed, I placed a quantity of the powder on an inclined plane, and by exposing it cautiously, and repeatedly, to a jet of water, I succeeded in washing away most of the grains of sand, and thus obtained it in a state of tolerable purity.

iron-sand and iserine.

The first of these minerals we may call *iron-sand*, and the second *iserine*, as they belong to mineral species, which oryctognosts have distinguished by these names.

The iron-sand described.

The iron-sand is much smaller in quantity than the iserine, and does not exceed one fourth of the mixture at most. Its colour is iron-black. It is in very small angular grains, commonly pretty sharp edged, and sometimes having the shape of imperfect octaedrons. The surface is rough; the lustre is feebly glimmering and metallic; the fracture, from the smallness of the grains, could not be accurately ascertained, but it seemed to be conchoidal. Opake, semihard, brittle, easily reduced to powder. Powder has a grayish black colour; powerfully attracted by the magnet; specific gravity 4·765.

Analysis of it.

1. As acids were not found to act upon this mineral, 100 grains of it were reduced to a fine powder, mixed with twice

twice its weight of carbonate of potash, and exposed for two hours to a red heat, in a porcelain crucible. The mass, being softened in water, was digested in muriatic acid. By repeating this process twice, the whole was dissolved in muriatic acid, except a brownish white matter, which, being dried in the open air, weighed 19½ grains.

Oxide of titanium.

2. The muriatic acid solution, which had a deep yellowish brown colour, was concentrated almost to dryness, and then diluted with water. It assumed a milky appearance; but nothing was precipitated. Being boiled for some time, and then set aside, a curdylike matter fell. It was of a milk-white colour, weighed, when dry, 7 grains, and possessed the properties of oxide of titanium.

Oxide of iron.

3. The residual liquid being supersaturated with ammonia, a dark reddish-brown matter precipitated, which being separated by the filter, dried, drenched in oil, and heated to redness, assumed the appearance of a black matter, strongly attracted by the magnet. It weighed 93·7 grains, and was oxide of iron.

4. The 19·5 grains of residual powder, being mixed with four times its weight of carbonate of soda, and exposed for two hours to a red heat, in a platinum crucible, and afterward heated with muriatic acid, was all dissolved, except about a grain of blackish matter, which was set aside.

More oxide of titanium.

5. The muriatic solution being concentrated by evaporation, a little white matter was separated. It weighed one fourth of a grain, and possessed the characters of oxide of titanium.

Silex.

6. When evaporated to dryness, and redissolved in water, a white powder remained, which proved to be silica, and which, after being heated to redness, weighed one grain.

Alumine.

7. The watery solution being supersatured with potash, and boiled for a few minutes, was thrown upon a filter, to separate a reddish-brown matter, which had been precipitated. The clear liquid, which passed through the filter, was mixed with a solution of sal ammoniac. A soft white matter slowly subsided. It was alumina, and, after being heated to redness, weighed half a grain.

8 The

More oxide of titanium.

8. The brown-coloured matter which had been precipitated by the potash, when dried upon the steam-bath, weighed 20·2 grains. It dissolved with effervescence in muriatic acid. The solution had the appearance of the yolk of an egg. When boiled for some time, and then diluted with water, it became white, and let fall a curdy precipitate, which weighed, when dry, 4·6 grains, and possessed the properties of oxide of titanium.

Arsenic.

9. The residual liquor, being mixed with an excess of ammonia, let fall a brown matter, which, after being dried, drenched in oil, and heated to redness, weighed 6 grains. It was strongly attracted by the magnet, but was of too light a colour to be pure oxide of iron. I therefore dissolved it in muriatic acid, and placed it on the sand-bath, in a porcelain capsule. When very much concentrated by evaporation, small white needles began to make their appearance in it. The addition of hot water made them disappear; but they were again formed, when the liquor became sufficiently concentrated. These crystals, when separated, weighed 1·3 grains, and proved, on examination, to be white oxide of arsenic. During the solution of the 6 grains in muriatic acid, a portion of black matter separated. It weighed 0·2 of a grain, and was totally dissipated before the blowpipe in a white smoke. Hence, it must have been arsenic. These 1·5 gr. are equivalent to rather more than 1 grain of metallic arsenic. Thus it appears, that the 6 grains contained 1 grain of arsenic, which explains the whiteness of their colour. The rest was iron. It can scarcely be doubted, that the proportion of arsenic present was originally greater. Some of it must have been driven off, when the iron oxide was heated with oil.

Iron.

More oxide of titanium.

10. The insoluble residue (No. 4) was with great difficulty dissolved in sulphuric acid. When the solution was mixed with ammonia, a white powder fell, which weighed 0·8 of a grain. It was accidentally lost, before I examined its properties. But I have no doubt, from its appearance, that it was oxide of titanium.

Results of the analysis.

11. Thus, from the 100 grains of iron-sand, the following constituents have been extracted by analysis:

Black

Black oxide of iron	98·70
White oxide of titanium.........	12·65
Arsenic	1·00
Silica and alumina	1·50
Total,	113.85

Here there is an excess of nearly 14 grains, owing, without doubt, to the combination of oxigen with the iron and the titanium during the analysis.

State of the iron in the ore.

Had the iron in the ore been in the metallic state, the excess of weight, instead of 14, could not have been less than 30. For the black oxide is known to be a compound of 100 metal and 37 oxigen. Hence, I think, it follows, that the iron in our ore must have been in the state of an oxide, and that it must have contained less oxigen than black oxide of iron. A good many trials, both on iron-sand, and on some of the other magnetic ores of iron, induce me to conclude, that the iron in most of them is combined with between 17 and 18 *per cent* of oxigen. This compound, hitherto almost overlooked by chemists, I consider as the real protoxide of iron. Thenard has lately demonstrated the existence of an oxide intermediate between the black and the red; so that we are now acquainted with four oxides of this metal. But the protoxide, I presume, does not combine with acids like the others. Analogy leads us to presume the existence of a fifth oxide, between the green and the red.

Protoxide of iron.

A fifth oxide presumable.

State of the titanium in the sand.

As to the titanium, it is impossible to know what increase of weight it has sustained, because we are neither acquainted with it in the metallic state, nor know how much oxigen its different oxides contain. It is highly improbable, that, in iron-sand, the titanium is in the metallic state, if it be made out that the iron is in that of an oxide. The experiments of Vauquelin and Hecht, compared with those of Klaproth, have taught us, that there are three oxides of titanium, namely, the blue, the red, and the white. From an experiment of Vauquelin and Hecht, and from some of my own, I am disposed to consider these oxides as composed of the following proportions of metal and oxigen;

1. Blue

Proportions of its oxides.

	METAL*.	OXIGEN.
1. Blue,	100	16
2. Red,	100	33
3. White,	100	49

I find, that, when the white oxide of titanium is reduced to the state of red oxide, it loses one fourth of its weight; and that red oxide, when raised to the state of white oxide, increases exactly one third of its weight. It was the knowledge of these facts, that led me to the preceding numbers. And I think they may be used, till some more direct experiments lead us to precise conclusions.

State of the titanium in the iron-sand.

Red oxide being the only state in which this metal has yet occurred separate, we may conclude, that it combines, in this state, with metallic oxides, and that the titanium in iron-sand, is most probably in this state. But white oxide, diminished by one fourth, gives us the equivalent quantity of red oxide. On this supposition, the titanium present, before the analysis, in the 100 grains of ore, weighed 9·5 grains.

The arsenic from accidental arsenic pyrites.

The appearance of the arsenic surprised me a good deal, as it was altogether unexpected. I am disposed to ascribe it to some particles of arsenic pyrites which might have been accidentally present. This conjecture will appear the more probable, when we reflect, that arsenic pyrites very frequently accompanies iron-sand. Before the microscope, the iron-sand appears to contain some white shining particles, which, probably, are arsenic pyrites.

The small quantity of silica and alumina I ascribe, without hesitation, to grains of quartz and felspar, which had adhered to the iron-sand, and been analysed along with it. Some such grains were actually observed and separated. But others, probably, escaped detection.

12. If these suppositions be admitted as well founded, the iron-sand was composed of

Component parts of the iron-sand.

Protoxide of iron	85·3
Red oxide of titanium	9·5
Arsenic	1·0
Silica and alumina	1·5
Loss	2·7
	100·0

* Probably, from what follows, an errour of the press for OXIDE. C.

The

The loss will not appear excessive, if we consider, that a portion of the arsenic must have been sublimed, before the presence of that metal was suspected.

Upon the whole, I think we may consider the specimen of iron-sand examined, as composed of 9 parts protoxide of iron, and 1 of red oxide of titanium. The presence of titanium in this ore had been already detected by Lampadius, though, as I have not seen his analysis, I cannot say in what proportion.

The colour of the iserine is iron-black, with a shade of brown. It consists of small angular grains, rather larger than those of the iron-sand, but very similar to them in their appearance. Their edges are blunt; they are smoother, and have a stronger glimmering lustre than those of the iron-sand. Lustre semimetallic, inclining to metallic. The fracture could not be distinctly observed, but it seemed to be conchoidal; at least nothing resembling a foliated fracture could be perceived. Opake, semihard, brittle, easily reduced to powder; colour of the powder unaltered; specific gravity 4·491*; scarcely attracted by the magnet. The iserine described.

1. A hundred grains of the powdered ore were mixed with six times their weight of carbonate of soda, and exposed for two hours to a red heat, in a platinum crucible. The mass obtained, being softened with water, dissolved completely in muriatic acid. When the solution was concentrated, it assumed the appearance of the yolk of an egg. It was boiled, diluted with water, and set aside for some time. A white matter gradually deposited, which, when dried on the steam-bath, weighed 53 grains, and possessed the properties of oxide of titanium. Analysis of it. Oxide of titanium.

2. The liquid, thus freed from titanium, was evaporated to dryness, and the residue redissolved in water, acidulated with muriatic acid. A white powder remained, which, after being heated to redness, weighed 16·8 grains, and possessed the properties of silica. Silex.

3. The solution was precipitated by ammonia, and the brown matter, which had separated, boiled for some time Alumine.

* If, as the following analysis would lead us to expect, the specimen examined was a mixture of four parts iserine, and one part quartz and felspar, the specific gravity of pure iserine should be 4·964.

in

in liquid potash. The whole was then thrown on a filter, to separate the undissolved part; and the liquid, which came through, was mixed with a solution of sal ammoniac. A white powder fell, which, after being heated to redness, weighed 3·2 grains. It was alumina.

Iron.

4. The brown substance collected on the filter was dried, drenched in oil, and heated to redness. It was strongly attracted by the magnet, and weighed 52 grains.

5. It was digested in diluted sulphuric acid; but not being rapidly acted upon, a quantity of muriatic acid was added, and the digestion continued. The whole slowly dissolved, except a blackish matter, which became white when exposed to a red heat, and, as far as I could judge from its properties, was oxide of titanium, slightly contaminated with iron. It weighed 1·8 grain.

More oxide of titanium.

6. The acid solution being concentrated by gentle evaporation, a number of small yellowish-coloured needles made their appearance in it. By repeated evaporations, all the crystals that would form were separated. They weighed 6 grains. I redissolved them in water, and added some ammonia to the solution. A fine yellow powder fell, which I soon recognized to be oxide of uranium. It weighed 4·2 grains.

Oxide of uranium.

7. Thus it appears, that the 52 grains (No. 4), attracted by the magnet, contained 46 grains of iron, and 6 grains of uranium and titanium.

Results of the analysis.

8. The following are the substances separated from 100 grains of iserine, by the preceding analysis:

Oxide of titanium	54·8
Oxide of iron	46·0
Oxide of uranium	4·2
Silica	16·8
Alumina	3·2
Total,	125·0

Here is an excess of no less than 25 grains, to be accounted for by oxigen, which must have united to the three metals during the process. As to the silica and alumina, there can be little hesitation in ascribing them to grains of sand, which had been mixed with the ore. The pure iserine, in all

all probability, was composed of iron, titanium, and uranium. If we suppose that each of these metals existed in the state of protoxide, we must diminish the titanium by one fourth, the iron by one seventh nearly, and the uranium, according to Bucholz's experiments, by one fifth. This would give us,

Titanium,	41·1*
Iron,	39·4
Uranium,	3·4
Silica and alumina,	20·0
	103·9

Here, then, is still an excess of nearly 4 *per cent.* But this I am disposed to ascribe to the oxides of titanium and uranium having been only dried upon the steam bath. Upon the whole, it appears, that in the specimens of iserine analysed, the proportions of titanium and iron were nearly equal, and that the uranium did not exceed 4 *per cent.* The appearance of uranium surprised me a good deal. I perceive, however, that it has already been detected in this ore, from an analysis published by Professor Jameson, in the second volume of his Mineralogy, which, I understand, was made by Lampadius. The specimen examined by Lampadius yeilded very nearly 60 parts of titanium, 30 of iron, and ten of uranium. Whereas, in mine, if the foreign matter be removed, there was obtained, very nearly, Component parts.

48 titanium,
48 iron,
4 uranium,

100

But there can be no doubt, that the iserine which I analysed was still contaminated with a good deal of iron sand; for it was impossible to remove the whole.

Analysis of a Gray Copper Ore, from Airthrey.

The copper mine of Airthrey, near Stirling, consists of a thin vein, which runs through the west corner of the Ochils. It has been twice wrought, by two different companies; Copper mine of Airthrey.

* This calculation supposes it in the state of deutoxide. As protoxide it would not exceed 33·3. C.

panies. But, in both cases, was abandoned, after a few years trial. I went to it some years ago, and examined the ore, at the request of one of the proprietors. The specimens, which were employed for the subsequent analysis, were the purest that I could select out of a considerable quantity. I was told, however, that from the lower level, which was at that time full of water, much richer ore had been extracted. But, afterward, when the lower level was freed from its water, I went down to it myself, and found the ore precisely of the same kind as in the upper, with this difference, that it was more mixed with a calcareous spar, and perhaps, on that account, more easily smelted.

Veinstones. The veinstones in the Airthrey mine are sulphate of barytes, and carbonate of lime, and with these the ore is almost always more or less mixed.

The ore described. The colour is at first a light steel gray; but the surface soon tarnishes, and becomes of a dark dull leaden gray, and in some places assumes a beautiful tempered steel tarnish. Massive and disseminated. In some specimens, it exhibits the appearance of imperfect crystals. Internal surface shining and metallic; but, by exposure, it soon becomes dull. Fracture small grained, inclining to even. Fragments indeterminate, and rather blunt edged. Semihard, the degree being almost the same as that of calcareous spar; for these two minerals reciprocally scratch each other. Streak similar, opake, brittle, easily frangible, specific gravity 4·878.

The ore first picked, 1. To free the ore as completely as possible from foreign matter, it was reduced to a coarse powder, and carefully picked. It was then digested in diluted muriatic acid, which dissolved a quantity of carbonate of lime, amounting to 13 *per cent* of the original weight of the ore.

then analysed. 2. Thus purified, it was dried on the steam bath, and 100 grains of it were reduced to a fine powder, and digested in diluted nitric acid, till every thing soluble in that menstruum was taken up. The residue was digested in the same manner in muriatic acid; and when this acid ceased to act, the residue was treated with nitro muriatic acid, till no farther solution could be produced. The insoluble matter was of a white colour; it weighed 6·9 grains, and

and was almost entirely sulphate of barytes. No traces of sulphate of lead, or of oxide of antimony could be detected in it by the blow pipe.

3. The three acid solutions being mixed together, no cloudiness appeared, nor was any change produced; a proof that the ore contained no silver. No silver in it.

4. The solution, being evaporated nearly to dryness, was diluted with water, and precipitated by muriate of barytes. By this means, the sulphuric and arsenic acids, which had been formed during the long continued action of the nitric acid on the ore, and the presence of which had been indicated by reagents, were thrown down; for nitrate of lead, added to the residual liquid, occasioned no precipitate; a proof that no arsenic acid was present.

5. The liquid, thus freed from arsenic acid, was mixed with an excess of ammonia. It assumed a deep blue colour, while a brown matter precipitated. It was separated by the filter, and being dried, drenched in oil, and heated to redness, it was totally attracted by the magnet. It weighed 45·5 grains, and was iron. Iron.

6. The ammoniacal liquid was neutralised by sulphuric acid, and the copper thrown down by means of an iron plate. It weighed 17·2 grains. Copper.

7. To ascertain the quantity of sulphur and arsenic, 100 grains of the purified ore, in the state of a fine powder, were put into the bottom of a coated glass tube, and exposed for two hours to a red heat. When the whole was cold, and the bottom of the tube cut off, the ore was found in a round solid mass, having the metallic lustre, a conchoidal fracture, and the colour and appearance of *variegated copper-ore*. It had lost 16 grains of its weight. Sulphur and arsenic.

8. The upper part of the tube was coated with a yellowish brown substance, like melted sulphur. It weighed 12·6 grains. Thus, there was a loss of 3·4 grains. As the tube was long, this loss can scarcely be ascribed to sulphur driven off. I rather consider it as water. For towards the beginning of the process, drops of water were very perceptible in the tube. Whether this water was a constituent of the ore, or derived from the previous digestion in muriatic acid, cannot be determined. Sulphur.

9. When

Arsenic.

9. When the 12·6 grains of yellowish brown matter detached from the tube were digested in hot potash-ley, the whole was dissolved, except a fine blackish powder, which weighed 1 grain, and was arsenic. The dissolved portion I considered as sulphur.

10. The potash solution, being mixed with nitric acid, 4 grains of sulphur fell. The remaining 7·6 grains must have been converted into sulphuric acid, by the action of the nitric acid. Accordingly, muriate of barytes occasioned a copious precipitate.

11. The 84 grains of roasted ore being reduced to a fine powder, mixed with half their weight of pounded charcoal, and roasted a second time in a glass tube, one grain of sulphur sublimed. But the tube breaking before the roasting had been continued long enough, the process was completed in a crucible. The roasted ore weighed 70 grains.

Component parts of the ore.

12. From the preceding analysis, we learn that the constituents of the Airthrey ore are as follows:

Iron,	45.5
Copper,	17·2
Arsenic,	14·0
Sulphur,	12·6
Water,	3·4
Foreign bodies,	6.9
	99·6
Loss,	·4
	100·0

If we suppose the water and the earthy residue to be only accidentally present, then the only essential constituents are the first four, and the ore would be a compound of

Iron,	51·0
Copper,	19·2
Arsenic,	15 7
Sulphur,	14·1
	100·0

Klaproth's analyses.

If we compare this analysis with several analyses of gray copper ore, lately published by Klaproth, we shall find, that the constituents are the same in both; but the proportions

tions of the first two ingredients are very nearly reversed. Klaproth obtained from 0·4 to 0·5 of copper, and from 0·22 to 0·27 of iron. This renders it obvious, that the two ores were not in the same state. I have little doubt, that the difference, however, is merely apparent, and that it arose, altogether, from a quantity of iron pyrites, and perhaps also of arsenic pyrites, which I could not separate from the gray copper ore which I examined. Both of these minerals could be distinctly seen in many of the specimens, intimately mixed with the gray copper; and I have no doubt, that the same mixture existed, even in those specimens which were selected as purest. The difference in the proportions of copper and arsenic, obtained by Klaproth* in his various analyses, is so considerable, as to lead to a suspicion, that even his specimens, in all probability, contained a mixture of foreign matter.

IV.

Researches on the Oximuriatic Acid, its Nature and Combinations; and on the Elements of the Muriatic Acid. With some Experiments on Sulphur and Phosphorus, made in the Laboratory of the Royal Institution. By H. DAVY, *Esq. Sec. R. S. Prof. Chem. R. I. F. R. S. E.*

(Concluded from vol. XXVII, p. 337.)

I SHALL occupy the time of the society for a few minutes only, for the purpose of detailing a few observations connected with the Bakerian lectures delivered in the last two years; particularly those parts of them relating to sulphur and phosphorus, which new and more minute inquiries have enabled me to correct or extend.

I have already mentioned, that there are considerable differences in the results of experiments made on the action of

Differences in the results of

* Gehlen's Jour. vol. V, p. 9, 11, 13.

experiments on sulphur and phosphorus.

of potassium on sulphur and phosphorus, and their combinations with hidrogen, according to different circumstances of the process. I shall now refer to such of these circumstances, as I have been fully able to investigate.

Sulphur contains an acid,

even when sublimed in nitrogen.

The able researches of Dr. Thompson have shown, that sulphur, in its usual state, contains small quantities of acid matter; and though, in my first experiments, I conceived that by employing crystallized native sulphur, which had been recently sublimed in nitrogen, I should avoid the presence of any foreign matter, yet I am inclined to believe, that this is not the case; for, by subliming some similar sulphur in nitrogen, I find, that litmus paper placed in the upper part of the retort is slightly reddened.

Retort must be lined with sulphur when combining with potassium.

Sulphuretted hidrogen evolved from it underrated.

When potassium is made to unite with sulphur, if the retort employed is not lined with sulphur, some of the potassium is destroyed by acting upon the glass; and when large quantities of sulphur are used, it is very difficult to decompose the whole of the sulphuret of potassium by an acid: sulphuretted hidrogen likewise is soluble in muriatic acid; and this circumstance led me to underrate the quantity of sulphuretted hidrogen given off in experiments of this kind*.

Another errour pointed out.

In acting upon sulphuretted hidrogen by potassium in my early experiments, I used large quantities of the gas and of the metal; and in these cases I have reason to believe, that the violence of the combustion occasioned the decomposition of a considerable quantity of the gas; and, in consequence, led me to form erroneous conclusions concerning the nature of this curious operation.

More accurate mode of conducting these

In all late experiments, in which sulphur, or sulphuretted hidrogen was concerned, I have used muriatic acid saturated

* This circumstance has been pointed out by Messrs Gay-Lussac and Thenard, in a paper printed in the Journal de Physique for December, in which these gentlemen endeavour to show, that, whether potassium has been acted upon by large or small quantities of sulphur, and under all circumstances, it evolves a quantity of gas exactly equal to that which it produces by the action of water. I have been able to gain no results so precise on this subject. I have in another place (the same journal in which their memoir has appeared) offered some observations on their inquiries.

rated with sulphuretted hidrogen over mercury. I have employed sulphur distilled from iron pyrites in vacuo, which did not in the slightest degree affect litmus paper, and I have combined it with potassium in retorts of green glass, or plate glass lined with sulphur, and filled with very pure nitrogen or hidrogen. In making potassium act upon sulphuretted hidrogen, I have employed the gas only in the quantities of from 1 to 3 cubical inches, and have made the combination in narrow curved tubes of green glass over dry mercury. With all these precautions, and after having made a great number of experiments, I am not able to gain perfectly uniform results. Yet there is a sufficient correspondence between them, to enable me to form conclusions, which I may venture to say cannot be far from the truth.

experiments. Sulphur sublimed from pyrites in vacuo not acid.

Results not perfectly uniform.

When 1 grain of potassium, which would give by the action of water about 1 cubical inch and $\frac{1}{10}$ of hidrogen, is made to act upon about half a grain of sulphur, some sulphur sublimes during the combination, which always takes place with heat and light; and from $\frac{1}{14}$ to $\frac{1}{10}$ of a cubical inch of sulphuretted hidrogen is evolved. The compound acted on by muriatic acid, saturated with sulphuretted hidrogen, affords from $\frac{9}{10}$ to $\frac{1}{1}\frac{1}{0}$ of a cubical inch of pure sulphuretted hidrogen.

Action of potassium on sulphur, in small proportion,

When more sulphur is used, so as to be from twice to ten times the weight of the potassium, the quantity of sulphuretted hidrogen evolved by the action of the acid is from $\frac{7}{10}$ to $\frac{9}{10}$; but if heat be applied to the combination, so as to drive off the superfluous sulphur, the quantity of gas collected is very little inferior to that produced from the combination in which a small proportion of sulphur is used; and I am inclined to believe, from the phenomena presented in a great number of experiments, that sulphur and potassium, when heated together under common circumstances, combine only in one proportion, in which the metal is to the sulphur nearly as 3 to 1 in weight; and in which the quantities are such, that the compound burns into neutral sulphate of potash.

and in larger proportion.

They combine only in one proportion.

When a grain of potassium is made to act upon about 1·1 cubical inches of sulphuretted hidrogen, all the hidrogen is

Action of potassium on sulphuretted hi-

drogen, in due proportion; is set free, and a sulphuret of potassium, containing one fourth of sulphur, is formed, exactly the same as that produced by the immediate combination of sulphur and the metal.

and in larger proportion. When sulphuretted hidrogen is employed in larger quantities, there is an absorption of this gas, and a volume is taken up about equal to the quantity of hidrogen disengaged; and a compound of sulphuretted hidrogen and sulphuret of potassium is formed, which gives sulphuretted hidrogen by the action of an acid, nearly double in quantity to that given by the sulphuret of potassium.

Potassium combines with phosphorus only in one proportion. From a number of experiments, I am inclined to believe, that potassium and phosphorus, in whatever quantities they are heated together, combine only in one proportion, a grain of potassium requiring about $\frac{3}{8}$ of a grain of phosphorus to form a phosphuret; which, when acted upon by muriatic acid, produces from $\frac{8}{10}$ to $\frac{10}{10}$ of a cubical inch of phosphuretted hidrogen.

Action of potassium on phosphuretted hidrogen. Half a grain of potassium decomposes nearly three cubical inches of phosphuretted hidrogen, and sets free rather more than four cubical inches of hidrogen; and the phosphuret formed seems to be of the same kind as that produced by direct combination of the metal with phosphorus.

Weight of an ultimate atom of sulphur deduced from its compounds. If, according to Mr. Dalton's ideas of proportion, the quantity in which sulphur enters into its combinations were to be deduced from its union with potassium, in which it seems to form about $\frac{1}{4}$ the weight of the compound, the number representing it would be 13·5. I have lately weighed sulphuretted hidrogen, and sulphureous acid gas, with great care: the specific gravity of the first at mean temperature and pressure, from my experiments, is 10645, which differs very little from the estimation of Mr. Kirwan: that of sulphureous acid gas I find is 20967. Sulphuretted hidrogen, as I have shown, contains an equal volume of hidrogen; and on this datum the number representing sulphur is 13·4. I have never been able to burn sulphur in oxigen without forming sulphuric acid in small quantities; but in several experiments I have obtained from 92 to 98 parts of sulphureous acid from 100 of

Sp. grav. of sulphuretted hidrogen and sulphureous acid gas.

of oxigen in volume; from which I am inclined to believe, that sulphureous acid consists of sulphur dissolved in an equal volume of oxigen; which would give the number as 13·7* nearly, considering the acid gas as containing 1 portion of sulphur, and 2 of oxigen; and these estimations do not differ from each other materially.

Weight of an ultimate atom of phosphorus.

I have made several experiments on the combustion of phosphorus in oxigen gas. From the most accurate, I am inclined to conclude that 25 of phosphorus absorb in combustion about 34 of oxigen in weight; and considering phosphoric acid as composed of 3 proportions of oxigen and 1 of phosphorus, the number representing phosphorus will be about 16·5, which is not very remote from the number that may be deduced from the composition of phosphoret of potassium.

Sulphur and phosphorus may contain oxigen and hidrogen combined in them,

The numbers, which represent the proportions in which sulphur and phosphorus unite with other bodies, are such, as do not exclude the existence of combined portions of oxigen and hidrogen in their constitution; but it may be questioned, whether the opinion which I formed, that the inflammable gas disengaged from them by electricity is necessary to the peculiar form in which these bodies exist, is not erroneous. Phosphorus, as I have stated in the last Bakerian lecture, is capable of forming a solid hydruret: and a part of the sulphur distilled from iron pyrites is usually of a soft consistence, and emits the smell of sulphuretted hidrogen, and probably contains that body. It is not unlikely, that in all cases phosphorus and sulphur

and probably in no case perfectly free

* The estimation from the composition of sulphuretted hidrogen must be considered as most accurate, and that from the formation of the sulphuret of potassium as least accurate: for it was only by combining sulphur and potassium in small proportions, and ascertaining in what cases uncombined sulphur could be distilled from the compound, that I gained my conclusions concerning the composition of the sulphuret of potassium.

In the last Bakerian lecture, I have estimated the specific gravity of sulphuretted hidrogen at 35 grains the 100 cubical inches, which was not far from the mean, between the estimations of Mr Kirwan and Mr. Thenard. According to this experiment, sulphuretted hidrogen is composed of 1 portion of hidrogen, represented by 1, and 1 of sulphur represented by 13 4.

 contain

from their hydrurets. contain small quantities of the hydrurets of phosphorus and sulphur; and the production of a minute portion of sulphuric acid in the slow combustion of sulphur is probably connected with the production of water.

Pure oxides of them never yet obtained. Though the pure oxides of sulphur and phosphorus have never been obtained, yet, from the doctrine of definite proportions, these bodies ought, under certain circumstances, to be formed. And I am inclined to believe, that they sometimes exist in minute quantities, in common phosphorus and sulphur, and, with hidrogen, give to them their variable properties.

Phosphorus differs in colour, probably from a mixture of oxide. The colours of different specimens of phosphorus, as well as of sulphur, differ considerably; the red colour of phosphorus, as it is commonly prepared, is probably owing to a slight mixture of oxide.

So does sulphur. Common roll sulphur is of a very pale yellow; the Sicilian sulphur of an orange colour; and the sulphur distilled from iron pyrites in vacuo, which arose in the last period of the process, of a pale yellowish green colour. All the late experiments that I have made, as well as my former researches, induce me to suspect a notable proportion of oxigen in Sicilian sulphur; which is probably owing to the presence of oxide of sulphur, which may give rise to sulphuric acid in distillation; or to sulphuric acid itself.

Action of oximuriatic acid gas Conceiving, that, if definite proportions of oxigen and hidrogen existed in sulphur and phosphorus, they ought to be manifested in the agency of oximuriatic acid gas on these bodies, I made some experiments on the results of these operations.

on sulphur, In the first trial, on the combination of sulphur with oximuriatic acid gas, I employed 5 grains of roll sulphur, and admitted the gas into the exhausted retort, from a vessel in which it had been in contact with warm water: in this case more than half a cubical inch of oxigen gas, and nearly two cubical inches of muriatic acid gas, were produced. Suspecting in this instance, that aqueous vapour had been decomposed, I employed cold water in the next experiment, and dried the gas by muriate of lime; in this case, though Sicilian sulphur was used, no oxigen gas was evolved; and not half a cubical inch of muriatic acid; the quantity was the same as in the last experiment;

experiment; and it was found, that between 16 and 17 cubical inches of oximuriatic acid gas disappeared; the whole of the sulphur was sublimed in the gas, and the liquor formed was of a tawny orange colour.

No oxigen was expelled during the combustion of phosphorus in oximuriatic acid gas; nor could I ascertain, that any muriatic acid had been formed. Three grains of phosphorus were entirely converted into sublimate, by the absorption of about 23 cubical inches and a half of the gas. and on phosphorus.

It would seem from these quantities, that the sulphuretted liquid, formed by subliming sulphur in oximuriatic acid gas, consists of 1 proportion of sulphur, represented by 13·5, and 1 of oximuriatic gas represented by 32·9, and that the phosphoric sublimate must be composed of 3 portions of oximuriatic gas, represented by 98·7, and 1 of phosphorus represented by 16·5. Proportion of the compounds.

V.

On the Culture of Parsneps, and their Utility in Feeding Cattle: by CHARLES LE HARDY, *Esq., of the Island of Jersey**.

SIR,

HAVING observed in the book of premiums offered by the society, that they wished for information on the culture of parsneps, which are much used in the island of Jersey; as having practised it for many years, I take the liberty to communicate what I know on the subject, with the result of some comparative experiments. Culture of parsneps, in Jersey,

The culture of parsneps and beans is looked upon as one of the regular courses of crops in the island. There is no farmer, be the extent of his grounds ever so small, who does not yearly plant a proportionate quantity, for the purpose of fattening his hogs and cattle, or feeding his milch cows. for hogs and cattle.

* Trans. of the Society of Arts, vol. XXVII, p. 52. The silver medal was voted to Major Le Hardy for this communication.

A few

They answer better than potatoes.

A few years ago, the culture of potatoes was substituted by some farmers to that of parsneps, and apparently with advantage; but farther experience has brought them back again to their former practice. Potatoes produce more weight and measure on a given extent of ground, and may be cultivated with less expense; still the parsnep is found to answer best for the farmer's purpose. A perch of the island, which is twenty-four square feet, will produce on an average crop seven cabots of potatoes, each weighing forty pounds, the same extent in parsneps will only average six cabots, which weigh only thirty-five pounds each, making twenty* pounds weight in favour of the potatoes, but they are not so nutritious as parsneps.

Soil.

Mode of culture.

Parsneps will thrive almost any where, but better in a deep stiff loam. They are generally cultivated in the island after a crop of barley, in the following manner.—At the end of January, or the beginning of February, the soil, which requires for this purpose to be stirred from the bottom, is either dug with spades after a skimming plough, or with two ploughs of different shapes following one another. The latter of the two, invented some years ago by a farmer in the island, will go to a depth of fifteen inches. In both these ways, the neighbouring farmers assist each other: In the season, it is not uncommon to see forty or fifty men in one field digging after a plough. When the large plough is used, less men are required, but more strength of cattle: two oxen and six horses are the team generally used. Those days are reckoned days of recreation, and tend to promote social intercourse among that class of men.

Beans dibbled in, and then parsneps sown.

After the ground has been tilled in this way, it is coarsely harrowed, and a sufficient number of women are provided to plant beans. These are dibbled in rows three by three

. . . . . .

. . . . . . at the distance of five feet from row to row.

. . . . . .

Two women may plant one vergee in a day. Two vergees and a half being equal to an English acre. Three sextenniers of parsnep seed, (about ¼ of a Winchester bushel) are then sown upon each vergee, and the whole is finely harrowed.

* Seventy, if the preceding numbers be right. C.

This

This crop now requires no attendance till the month of May, when weeding becomes necessary. This is the most expensive part of the culture. It is generally done by hand, with a small weeding fork, and as the parsneps require to be kept very clean, the expense is proportionate to the quantity of weeds. This latter summer four women were employed twenty-eight days each in weeding about five vergees. I tried a few perches with the hand hoe, and thinned them like turnips; they proved finer than those which were hand weeded. In Guernsey they make use of the spade for this purpose.

Weeding, by hand, the hoe, or the spade.

In the beginning of September, the beans are pulled up from among the parsneps, and about the latter end the digging begins. The instrument used is the common three pronged fork. This work is done gradually as the cattle want them, till the ground requires to be cleared for sowing wheat; which after parsneps is generally done about the middle of December. They are reckoned an excellent fallow for that kind of grain, and the finest crops are generally those which succeed them; as it is a tap rooted plant, it does not, like the potato, impoverish the surface, but leaves it mellow, and free from weeds, to a succeeding crop.

Digging up. Followed by wheat, for which they are an excellent preparation.

When parsneps require to be kept for the use of cattle, they are brought under dry sheds, and will keep good without any care till the end of March. Should they require to be kept longer, they are laid in double rows over one another, their heads outward, with alternate strata of earth, which, when finished, have the appearance of small walls, or, if made circular, of small towers. Those for seed are always preserved in this manner, and sometimes carrots and beets for culinary purposes.

Mode of keeping them.

Parsneps are not injured by frost; after having been frozen, they are fit for vegetation; the only sensible alteration is their acquiring a sweeter taste, and by this perhaps becoming more nutritive. They are given raw to hogs and to horned cattle. Though horses are fond of these roots, they are not suffered to eat them, as they make them languid, and are apt to injure their sight. Their leaves, when

Not injured by frost. Injurious to horses.

The wet leaves caustic.

when wet, are so caustic as to blister the hands of the weeders, and sometimes to occasion a violent inflammation in the eyes and udders of the cattle feeding upon them.

Good food for milch cows.

Cows fed on parsneps in the winter months give a greater quantity of milk and butter, and of better flavour, than those fed upon potatoes. The butter is nearly equal to that from spring grass. Though the root of this plant has the quality of improving that article, it must be observed, that the leaves give it a very disagreeable taste, which however is of no consequence when intended to be potted, as it goes off in a short time.

Bad for breeding sows, but good to fatten hogs,

Parsneps are dangerous food for sows before they farrow, and might occasion them to lose their litter. Hogs may be fattened with them in about six weeks. It is the custom during that time, to thicken their swill with the meal of beans and oats ground together. Pork fattened in this way is very firm, and does not waste in boiling.

or horned cattle,

Horned cattle may be fattened with parsneps in about three months. I never knew them used for sheep.

which are much improved by them.

It is the general opinion in the island, that hogs or cattle fed on parsneps may be brought to a condition for slaughtering, in less time, and with half the quantity, that would be required of potatoes. The butchers are sensible of the superiority of the former, and will give a halfpenny per pound more for cattle fattened with them, than for such as have been fed any other way. Upon inquiry I was informed, they always contained a greater quantity of tallow.

This I believe to be a full account of the culture and use of the parsnep, and a just comparison with the potato.

I remain, Sir,

Your most obedient humble servant,

CHARLES LE HARDY.

VI.

VI.

Observations upon Luminous Animals. *By* J. MACARTNEY, *Esq.* *Communicated by* EVERARD HOME, *Esq. F. R. S.*

(Concluded from vol. XXVII, p. 350.*)*

THE remarkable property of emitting light during life is only met with among animals of the four last classes of modern naturalists, viz. mollusca, insects, worms, and zoophytes.

Only the 4 last classes of animals emit light while living.

The mollusca and worms contain each but a single luminous species; the *pholus dactylus* in the one, and the *nereis noctiluca* in the other.

Only 2 luminous species in the 1st and 3d.

Some species yield light in the eight following genera of insects; *elater, lampyris, fulgora, pausus, scolopendra, cancer, lynceus**, and *limulus*. The luminous species of the genera lampyris and fulgora are more numerous than is generally supposed, if we may judge from the appearance of luminous organs to be seen in dried specimens.

Genera of insects containing luminous species.

Among zoophytes we find, that the genera *medusa, beroe*†, and *pennatula*, contain species which afford light.

Genera of zoophytes.

The only animals, which appear to possess a distinct organization for the production of light, are the luminous species of lampyris, elater, fulgora, and pausus.

Animals with organs fur producing light.

The light of the lampyrides is known to proceed from some of the last rings of the abdomen, which, when not illuminated, are of a pale yellow colour. Upon the internal surface of these rings there is spread a layer of a peculiar soft yellow substance, which has been compared to paste; but by examination with a lens I found it to be organized like the common interstitial substance of the insect's body, except that it is of a closer texture, and a paler

Organ in the fireflies.

A peculiar substance,

* The animal discovered by Riville off the coast of Malabar, in 1755, is certainly a testaceous insect, and appears to belong to the genus lynceus of Muller.

† The luminous zoophite, for which Peron has instituted the new genus pyrosoma, appears to me to be a beroe, and only worthy of a specific distinction.

paler yellow colour. This substance does not entirely cover the inner surface of the rings, being more or less deficient along their edges, where it presents an irregular waving outline. I have observed in the glow-worm, that it is absorbed, and its place supplied by common interstitial substance, after the season for giving light is past.

existing at particular seasons.

The segments of the abdomen, behind which this peculiar substance is situate, are thin and transparent, in order to expose the internal illumination.

The number of luminous rings varies in different species of lampyris; and, as it would seem, at different periods in the same individual.

Two sacs in the glow-worm,

Beside the luminous substance above described, I have discovered in the common glow-worm, on the inner side of the last abdominal ring, two bodies, which to the naked eye appear more minute than the head of the smallest pin. They are lodged in two slight depressions, formed in the shell of the ring, which is at these points particularly transparent. On examining these bodies under the microscope, I found, that they were sacks, containing a soft yellow substance, of a more close and homogeneous texture, than that which lines the inner surface of the rings. The membrane forming the sacs appeared to be of two layers, each of which is composed of a transparent silvery fibre, in the same manner as the internal membrane of respiratory tubes of insects; except that in this case the fibre passes in a spiral, instead of a circular direction. This membrane, although so delicately constructed, is so elastic as to preserve its form, after the sac is ruptured, and the contents discharged.

which emit light almost constantly.

The light, that proceeds from these sacs, is less under the control of the insect, than that of the luminous substance spread on the rings: it is rarely ever entirely extinguished in the season that the glow-worm gives light, even during the day: and when all the other rings are dark, these sacs often shine brightly.

Said to vary in number,

The circumstance of there being points, which give a more permanent light than the other parts of the luminous rings of the abdomen, has been noticed before by the Comte

Comte G. de Razounmouski. He states the number of these luminous points to vary from 2 to 5*.

but probably this inaccurate.

I must however remark, that I never saw more than two of these luminous points, which were always upon the last ring of the body: and that the figures, which accompany the memoir of the Comte de Razoumouski, bear scarely any resemblance to the insect they are intended to represent, from which we may fairly suspect him of inaccuracy in other particulars.

They are not common to the genus.

As far as my observation has extended, the small sacs of luminous substance are not found in any species of lampyris, except the glow-worm of this country. Thunberg mentions, that the lampyris japonica has two vesicles on the tail, which afford light.

Organ in the springing beetles.

The organs for the production of light in the genus elater are situate in the corselet. These likewise consist of a peculiar yellow substance, placed behind transparent parts of the shell, which suffer the natural colour of this substance to be seen through them in the day, and, when illuminated, give passage to the light.

On dissecting the organs of light in the elater noctilucus, I found that there is a soft yellow substance, of an oval figure, lodged in the concavity of the yellow spots of the corselet, which parts are particularly thin and transparent in this species. This substance is so remarkably close in its structure that at first view it appears like an inorganic mass; but with a lens it is readily perceived to be composed of a great number of very minute parts or lobules closely pressed together. Around these oval masses the interstitial substance of the corselet is arranged in a radiated manner; and the portion of the shell, that immediately covers the irradiated substance, is in a certain degree transparent, but less so than that which lies over the oval masses; it is therefore probable, that the interstitial snbstance in this situation may be endowed with the property of shining. A fasciculus of the muscles of the corselet arises in the interior of the oval masses of the luminous substance, but not apparently with any design; as it contributes, with the adjacent fasciculi, to move the anterior feet.

* Mém. de la Soc. de Lausanne, Tom. ii.

In

In the elater ignitus, the masses of luminous substance are extremely irregular in their figure: they are situate nearly at the posterior angles of the corselet, and are more loose in their texture than the oval masses of the noctilucus, resembling rather in composition the interstitial substance, which surrounds these masses in that species. The shell of the corselet is somewhat thinner, and more transparent along both sides of the margin, than at other places; but it is not, as in the noctilucus, elevated, and peculiarly clear and thin, immediately over the seat of the luminous organ; consequently the light emitted by the elater ignitus cannot be very brilliant.

I have not been able to procure any specimen of the elater phosphorea: but from the accounts of naturalists it appears to resemble in every respect the elater noctilucus; indeed I have great doubts of the phosphorea being a distinct species.

Organ in the lanternfly.

I have had an opportunity of examining, preserved in a moist way, two species of fulgora, the candelaria and laternaria. The light in this genus has been observed to issue from the remarkable proboscis of the fore part of the head. This part has always been described by authors as hollow, or empty, which I have found to be perfectly correct; and what is more extraordinary, that the cavity communicates freely with the external air, by means of a chink or narrow aperture, placed on each side of the root of the proboscis. This projection is covered internally by a membrane, between which and the horny part, or shell, there appears to be interposed a pale reddish coloured soft substance, that is arranged in the candelaria in broad lines or stripes; but it is so thin, that I could not distinctly examine its structure, or absolutely determine, whether it should be considered as a substance intended to furnish the light of these insects, or the pigment upon which the colour of the proboscis depends.

Pausus spherocerus.

The globes of the antennæ constitute the organs of light in the pausus spherocerus. Dr. Afzelius, who discovered the luminous property in this species, compares them to lanterns spreading a dim phosphoric light*. The rarity

* Lin. Trans. Vol. IV.

of

of the insect put it out of my power to examine its structure, but from the form and situation of its organs of light, it is most probable they are constructed like those of the fulgoræ.

It has been conjectured by Carradori and others, that the lampyrides were enabled to moderate or extinguish their light, by retracting the luminous substance under a membrane; but neither in them, or any of the other luminous insects, have I found an apparatus of this sort. The substance furnishing the light is uniformly applied to corresponding transparent parts of the shell of the insect, whence it is not moved; indeed a membrane, if it did exist, would have but little effect in obscuring the light, and never could serve to extinguish it. The regulation of the kind and degree of the luminous appearance does not depend upon any visible mechanism; but, like the production of the light itself, is accomplished by some inscrutable change in the luminous matter, which in some animals is a simple operation of organic life, and in others is subject to the will.

The light regulated not by any visible mechanism,

but by some change in the luminous matter.

It is worthy of remark, that, in all the dissections I have made of luminous insects, I did not find, that the organs of light were better, or differently supplied with either nerves or air tubes, than the other parts of the body. The power of emitting light likewise exists in many creatures which want nerves, a circumstance strongly marking a difference between animal light, and animal electricity.

The light of insects unconnected with nervous energy.

With the exception of the animals above mentioned, the exhibition of light depends upon the presence of a fluid matter.

In other animals the luminous matter is fluid:

In the pholas dactylus, the luminous fluid is particularly evident, and in vast quantity; it is recorded by Pliny, that this fluid is like liquid phosphorus, and renders every object luminous, with which it comes into contact. Reaumur also found, that it was diffusible in water, or any other fluid, in which the animal might be immersed*.

as in the pholas dactylus;

The shining of the scolopendra electrica I have always observed to be accompanied by the appearance of an effu-

scolopendra electrica;

* Mém. de l'Acad. des Sc. 1712.

sion

sion of a luminous fluid upon the surface of the animal, more particularly about the head; which may be received upon the hand, or other bodies brought into contact with the insect at the moment; and these exhibit a phosphoric light for a few seconds afterward. This fluid, however, I never could discover in the form of moisture, even upon the clearest glass, although examined immediately with the most scrupulous attention by a lens: it must therefore be extremely attenuated.

nereis noctiluca;

The same appearance has been observed during the illumination of the nereis noctiluca by Fougeroux de Bondaroy*.

and others.

The animal discovered by Riville shed a blue liquor, which illuminated the water for a distance of two or three lines†.

Spallanzani relates, that the medusa, which he examined, communicated the property of shining to water, milk, and other fluids, on being rubbed or squeezed in them‡.

This fluid partial, or general.

The luminous fluid is in some instances confined to particular parts of the body, and in others is diffused throughout the whole substance of the animal.

In the scolopendra electrica, it appears to reside immediately under the integuments. In the lynceus discovered by Riville it is contained in the ovary. If I may judge from my own observations, every part of the body of the medusæ is furnished with this fluid, as there is no part I have not seen illuminated under different circumstances; but Spallanzani affirms, that it is only found in the large tentacula, the edges of the umbella, and the purse, or central mass; which he proved, he says, by detaching these parts successively, when they shone vividly, while the rest of the body neither gave light, nor communicated any luminous appearance to water§.

Seapen.

Spallanzani discovered a mucous luminous fluid in the plumule of the pennatula phosphorea||.

* Mém. de l'Acad. des Sc 1767.
† Mém. Etrang. de l'Acad. des Sc. Tom. iii.
‡ Spallanzani's Travels in the Two Sicilies, Vol. iv.
§ Memoria sopra le meduse fosforiche. Mem. della Soc. Ital. Tomo vii.
|| Mem. della Soc. Ital, Tomo ii.

The

The phenomenon of animal light has been attempted to be explained in different ways. By many persons it was formerly ascribed to a putrefactive process; but, since the modern theories of combustion became known, it has been generally believed to depend upon an actual inflammation of the lumous substance, similar to the slow combustion of phosphorus. Others have accounted for the luminous effect, by supposing the matter of light to be accumulated, and rendered latent under particular circumstances, and afterward evolved in a sensible form.

Theories of animal light.

The opinion of the light of living animals being the consequence of putrefaction is evidently absnrd, and contradictory to all observation on the subject. It has been proved by the experiments of Dr. Hulme and others, that even the luminous appearances of dead animals are exhibited only during the first stages of the dissolution of the body, and that no light is emitted after putrefaction has really commenced.

It cannot be owing to putrefaction.

Spallanzani, who was the most strenuous advocate for the phosphorescent nature of animal light, stated, that glow-worms shone more brilliantly when put into oxigen gas; that their light gradually disappeared in hidrogen or in azotic gas, and was instantly extinguished in fixed air; that it was also lost by cold, and revived by the application of a warm temperature. He conjectured, that the luminous matter of these insects was composed of hidrogen and carbonated hidrogen gas.

Arguments for its being phosphorescent by Spallanzani

Forster relates, in the Lichtenberg Magazine for 1783, that, on putting a lampyris splendidula into oxigen gas, it gave as much light as four of the same species in common air.

and Forster.

Carradori has made some experiments upon the lucciole, (lampyris italica) which led him to deny its phosphorescence. He found, that the luminous portion of the belly of the insect shone in vacuum, in oil, in water, and different liquids, and under different circumstances, where it was excluded from all communication with oxigen gas. He accounts for the result of Forster's experiment, by supposing, that the worm shone more vividly, because it was more animated in oxigen gas than in common air.

Arguments against it by Carradori.

Carradori

Supposed by him and Brugnatelli to be the matter of light.

Carradori adopts on this subject the doctrine of Brugnatelli, and ascribes the luminous appearances of animals to the condensation and extrication of light in particular organs, which had previously existed in combination with the substance of their bodies. He supposes the light to be originally derived from the food, or the atmospheric air taken into the body: in short, that certain animals have the peculiar property of gradually imbibing light from foreign bodies, and of afterward secreting it in a sensible form *.

Experiments by the author.

The following experiments, which I made upon this subject, would lead me to make different conclusions than those of the preceding authors.

Glow-worm in water luminous.

Exp. 1. A glow-worm was put into a glass of water, in which it lived nearly two hours, and continued to emit light as usual, until it died, when the luminous appearance entirely ceased.

The substance not luminous from dead,

Exp. 2. The luminous substance was extracted from the beforementioned glow-worm, and from others killed in different ways; but it afforded no light.

but luminous when taken from living animals.

Exp. 3. The sacs containing the luminous matter were cut from the bellies of *living* glow-worms, and shone uninterruptedly for several hours in the atmosphere; and after their light became extinct, it was revived by being moistened with water. Some of these were put into water in the first instance, in which they continued to shine unremittingly for 48 hours.

Not luminous by heat.

Exp. 4. The luminous substance of a glow-worm was exposed to a degree of heat, which would have been sufficient to inflame phosphorus, without increasing the brilliancy of its light; and farther, it could not be made to burn by being applied to a red hot iron, or to the flame of a candle.

A little heat apparently evolved during the shining of glow-worms,

Exp. 5. A delicate thermometer was introduced among some living glow-worms, during the time they gave out much light: the temperature of the room being 69, the instrument rose to 75, 76, and 77, according to circumstances, as the warmth was reflected from the hand, or dissipated

* Annal di Chimica, Tomo xiii. 1797.

sipated by the worm crawling over cold substances. The luminous portion of the tail, when very brilliant, appeared to raise the thermometer more quickly than the other parts of the body, but it was not invariably the case. When shining strongly, I thought that the luminous rings communicated the sensation of warmth to the hand, but this was probably a deception, as the actual degree of heat was not sufficient for such an effect. It should however be mentioned, that in Templar's observations on the glow-worm, he said his feelings deceived him, if he did not experience some heat from the shining of the insect*.

Exp. 6. To satisfy myself how far the evolution of heat during the shining of glow-worms depended upon the life of the animals, I cut off the luminous portion of the tail from several living worms; and I found, that, if the thermometer was applied to them immediately, it was raised by them one or two degrees; but after these parts were dead, although they continued to emit light, they produced no effect whatever upon the instrument.

but only while they were alive.

Exp. 7. Some hemispherical medusæ were put into a spoon, containing a small quantity of seawater, and held over a burning candle. As soon as the water became heated, the medusæ appeared like illuminated wheels, the spots at the margin and centre alone emitting light; in which manner they shone vividly and permanently for about 20 seconds, when they shrunk and died, after which they were no longer luminous.

Medusæ shone when heated in sea water,

Exp. 8. Some of the same species were put into spirits: a strong and unremitting light was instantly given out, which issued from the central and marginal parts, as in the preceding experiment, and continued until they died.

immersed in spirits,

Exp. 9. Some of the scintillating and hemispherical species of medusa, contained in a small glass jar, were introduced into the receiver of an air pump, and, the air being exhausted, they shone as usual when shaken; if any difference could be perceived, the light was more easily excited, and continued longer in vacuo.

or shaken in a vacuum:

* Phil. Trans. No. 72.

I wished next to try the influence of electricity on the luminous property of animals.

but not when electrified,

Exp. 10. A medusa hemispherica was placed in a small glass dish, containing a quantity of water, merely sufficient to allow the animal to preserve its figure; being insulated, it was electrified, and sparks drawn from it, which had not the slightest effect; the experiment was repeated several times with different individuals, but without exciting the animals to throw out light.

unless when slight shocks were passed through them.

Exp. 11. Some hemispherical medusæ were placed in contact with the two ends of an interrupted chain, and slight electric shocks passed through them. During the very moment of their receiving the shock no light was visible, but immediately after the medusæ shone like illuminated wheels, which appearance remained for some seconds. Upon the closest inspection with a magnifying glass, no contractile motion could be perceived to accompany the exhibition of the light. The application of electricity in this instance seems to have acted merely as a strong mechanic shock.

The above experiments on the luminous medusæ were made at Herne, with the assistance of George May, Esq., of Stroudhouse, and in the presence of a large company, capable of accurately distinguishing their results.

General deductions from these experiments.

It seems proved by the foregoing experiments, that, so far from the luminous substance being of a phosphorescent nature, it sometimes shows the strongest and most constant light, when excluded from oxigen gas; that it in no circumstances undergoes any process like combustion, but is actually incapable of being inflamed; that the increase of heat, during the shining of glow-worms, is an accompaniment, and not an effect of the phenomenon, and depends upon the excited state of the insect; and lastly, that heat and electricity increase the exhibition of light, merely by operating like other stimuli upon the vital properties of the animal.

The light not affected by any kind of gas.

In confirmation of these opinions, I may quote the high authority of the secretary of the society, who has found, that the light of the glow-worm is not rendered more brilliant in oxigen, or in oxigenated muriatic gas, than in common

common air; and that it is not sensibly diminished in hidrogen gas.

I may farther add, that Spallanzani's experiments of diffusing the luminous liquor of the medusa in water, milk, and other fluids, are in direct contradiction of his own theory; as is also the extinction of the light of these mixtures by the application of a high degree of heat.

Spallanzani's experiments against his own theory.

If the light emitted by animals were derived from their food, or the air they respire, as supposed by Carradori, the phenomenon should be increased or diminished, according to the quantity of food or air, that the creatures consume; but we do not find this to be the case; for in those situations where they are sometimes found to be the most luminous, they are deprived, in a great measure, of these assumed sources of their light.

Their light not derived from food or air,

In fact, the luminous exhibitions of living animals are not only independent of all foreign light, but are frequently deftroyed by the latter. I have always found the shining of the medusæ to cease upon the rising of the moon, or at the approach of day; and when out of the sea, I never could excite them to throw out light, until they had been kept for some time in the dark; all the luminous insects likewise secrete themselves as much as possible during the day time, and go abroad only at night. I have, it is true, found, that the scolopendra electrica will not shine, unless it has been previously exposed to solar light; but I have observed, that it shone as brilliantly and as frequently, after being kept a short time in a light situation, as when left uncovered the whole day. The circumstance of the scolopendra requiring exposure previous to its giving out light is very unaccountable; as the insect, when left to itself, always seeks as much as possible concealment during the day; indeed it is the opinion of some naturalifts, that it is killed by the light of the sun.

and independent of foreign light,

except perhaps in the scolopendra.

This exception a curious fact.

The opinions of Brugnatelli and Carradori are connected with some general doctrines, respecting the nature of light, which I shall not at present venture to discuss. It appears to me, that the question is still unresolved, whether light has a substantial existence, or is a phenomenon depending upon certain operations or conditions of the

Whether light be material not yet determined:

the ordinary forms of matter. But the highly ingenious researches of Count Rumford, on the laws of what have been called subtile fluids; and the extraordinary advances lately made by Mr. Davy, on the decomposition of substances, that were hitherto looked upon as elementary; give us reason to hope, that future investigations may unfold views of the material world, of which we can at present have only an indistinct conception; that new modes of analysis may enable us to see things, not "through a glass darkly," but more nearly as they are; and that the boundaries of physical and metaphysical science, now so far asunder, may be made to approach each other.

In the present state of our knowledge, our business should be, to collect, arrange, and compare phenomena, rather than to speculate upon their nature. Nevertheless, I cannot refrain from observing, that the circumstances attending the luminous appearance of living animals are much more favourable to the supposition of light being a property, than a substance. The quantity of light emitted by an animal in a certain time (admitting it to be matter) far exceeds that which could be possibly supplied by the sources, whence it is usually supposed to be derived. Thus the luminous appearance of some medusæ may be continued with the intermission of short intervals for an indefinite time, notwithstanding the creature be kept in darkness, and without any other food than what a small quantity of filtered seawater would afford. The uninterrupted and long continued light, that is sometimes evolved by the luminous sacs and the ova of the glow-worm, is also inconsistent with the notion of an accumulation and subsequest dispersion of a material substance.

But the phenomena of luminous animals are rather more favourable to the supposition of its being a quality.

I shall terminate this paper by an enumeration of the several conclusions, that are the result of the observations I have been able to make upon the phenomena of animal light.

General conclusions.

The property of emitting light is confined to animals of the simplest organization, the greater number of which are inhabitants of the sea.—The luminous property is not constant, but, in general, exists only at certain periods, and in particular states of the animal's body.—The power of showing

showing light resides in a peculiar substance or fluid, which is sometimes situate in a particular organ, and at others diffused throughout the animal's body.—The light is differently regulated, when the luminous matter exists in the living body, and when it is abstracted from it. In the first case, it is intermitting, or alternated with periods of darkness; is commonly produced or increased by a muscular effort; and is sometimes absolutely dependant upon the will of the animal. In the second case, the luminous appearance is usually permanent until it becomes extinct, after which it may be restored directly by friction, concussion, and the application of warmth; which last causes, operate on the luminous matter (while in the living body,) only indirectly, by exciting the animal.—The luminous matter, in all situations, so far from possessing phosphoric properties, is incombustible, and loses the quality of emitting light, by being dried, or much heated. The exhibition of light, however long it may be continued, causes no diminution of the bulk of the luminous matter. It does not require the presence of pure air, and is not extinguished by other gasses.

The luminous appearance of living animals is not exhausted by long continuance, or frequent repetitions, nor accumulated by exposure to natural light; it is, therefore, not dependent upon any foreign source, but inheres as a property, in a peculiarly organized animal substance or fluid, and is regulated by the same laws, which govern all the other functions of living beings.

Luminousness of the sea.

The light of the sea is always produced by living animals, and most frequently by the presence of the medusa scintillans. When great numbers of this species approach the surface, they sometimes coalesce together, and cause that snowy or milky appearance of the sea, which is so alarming to navigators. These animals, when congregated on the surface of the water, can produce a flash of light, somewhat like an electric corruscation. When the luminous medusæ are very numerous, as frequently happens in confined bays, they form a considerable portion of the mass of the sea, at which times they render the water heavier

heavier, aud more nauseous to the taste; it is therefore advisable always to strain seawater before it is drunk.

The luminous property does not appear to have any connection with the economy of the animals that possess it, except in the flying insects, which by this means discover each other at night, for the purpose of sexual congress.

Explanation of the Figures.

Explanation of the plates.

Fig. 1*. The cancer fulgens, discovered by the Right Hon. Sir Joseph Banks, of the natural size.

Fig. 2. The same animal magnified.

Fig. 3. The medusa pellucens, also found by Sir Joseph Banks, represented of the natural magnitude.

Fig. 4. The limulus noctilucus, discovered by Captain Horsburgh, considerably enlarged.

Fig. 5. The luminous medusa, discovered by me, which I conceive to be the medusa hemispherica: it is shown of the largest size I met with.

Fig. 6. The central process of this animal's body magnified, in order to explain its structure. The thick tentacula in which it terminates are seen covered with small cups or suckers.

Fig. 7. The beroe fulgens, discovered by me, shown in its most elongated or relaxed form, which it assumes commonly when swimming quickly.

Fig. 8. The same animal in the most contracted form.

Fig. 9. The minute species of medusa, discovered by me, which is the most frequent cause of the luminous appearance of the sea, represented of the natural size.

Fig. 10. The same animal magnified, exhibiting a puckered or tucked-in appearance on one side.

Fig. 11. Is the animalcule discovered by Forster, of the natural size.

Fig. 12. The same, greatly magnified, to show the intestinal parts. Both these figures are copied from the original drawings, in the possession of the Right Hon. Sir Joseph Banks.

* Figs 1, 2, and 3, will be found in plate IX of the preceding volume, the rest in plate II of this.

Fig. 13.

Fig. 13. Is an enlarged view of the inferior surface of the abdomen of the lampyris lucida, after the integument had been removed. *a a a* represent the three masses of luminous substance, which are applied to the three last rings of the abdomen. *b b b* the arrangement of the cellular or interstitial substance on the other abdominal rings, which gives the pale colour to the whole belly of this insect.

Fig. 14. Represents the common glow-worm, with the posterior portion on the back cut away to expose the sacs of luminous matter in situ on the last ring of the belly. *a* indicates the sac of one side; the intestine is seen to lie between them.

Fig. 15 and 16. Are the sacs of the glow-worm prodigiously magnified to show their structure. Fig. 16 is cut open to expose the luminous matter it contains: the coat of the sac is still seen to preserve its figure.

Fig. 17. Is the elater noctilucus, with the shell of the corselet removed on one side, by which the organ of light is uncovered. *a* the yellow transparent spot of the corselet. *b* the oval mass of luminous substance surrounded by an irradiation of the interstitial substance. *c* the ends of the muscles which were on the inside of the corselet.

Fig. 18. Is the posterior angle of the corselet of the elater noctilucus magnified. *a* the radiated appearance which the interstitial substance has round the oval mass of luminous matter. This mass is seen to consist of a number of smaller parts. *b* shows the appearance of the interstitial substance, where it passes down between the muscles. *c* the ends of the muscles of the back. *d* the shell of the corselet.

Fig. 19. Represents the elater ignitus. *a* is the mass of luminous substance of one side, seen indistinctly through the back of the semitransparent portion of the corselet. *b* is the luminous mass of the other side, exposed by removing a part of the shell of the corselet.

VII.

VII.

Hints on various Modes of Printing from Autographs. By G. CUMBERLAND, *Esq. In a Letter from the Author.*

To Mr. NICHOLSON.

SIR,

AS your Journal is frequently enlivened by hints of improvement in the Arts, no less than by accounts of new inventions; allow me, when it is agreeable to you, to occupy a page or two with some thoughts on a subject particularly interesting to authors; and which might, if reduced to practice successfully, be of general service to mankind. Every original writer justly laments the expense, difficulty, and fraud, he is subject to, if he gives his works to the press. If therefore any method could be devised, to enable a man of talents to be his own printer, and take off his own copies as they were demanded, without the intervention of a publisher, a new and brilliant era in the world of letters would be commenced; that would make thought and reflection, when justly employed, as valuable to the possessor as the talent for manual arts; and we might hope to see the day arrive, when the profession of letters might afford as probable a means of getting a fortune as any other profession whatever.

A mode of printing their own works would be very advantageous to men of letters.

In order to accomplish this desirable end, I have, long ago, run over in comparison every substance, that might possibly contribute to this purpose. Metals, wood, clay, paper, have not been forgotten; and although I cannot offer any one as having been fairly tried, (owing to a variety of other occupations), yet I trust I may, by enumerating them, afford hints to others, that may ultimately be useful.

Materials suggested.

My first idea was copper, written on by a stile through white wax, and when printed perused backwards by means of a mirror. *But this would have demanded the talents of a *Blake*, who alone excels in that art: or we must have a child

Copper.

* I apprehend there is an omission here of some such words as the following. "It might be written upon backward,"

child educated so to write with correctness. Besides, copper is too dear ever to become useful. Yet this method might do for short pieces, if furnished with Dr. *Lind's* press, (a useful instrument, that I believe has never yet been described or published), and I have practised it, but is too costly to answer for large works. Let us therefore suppose a kind of copper or brass latten to be rolled thin for the purpose, and the writer to use a very corrosive ink, which in a short time would eat quite through the whole body. He would by this means produce a stencil as fast as he could write, by means of which he would be enabled to print the right way.

Stencilling.

Again let us suppose he were to make use of capital letters only, acting as punches on paper, he would by this method have a paper stencil, that would last as long, perhaps longer, than the latten one.

Paper stencil.

Tin foil again might be used in this way, or fine plates of bismuth, for common ingenuity might overcome the difficulties of the O and other letters by ties. Many people will smile at the idea of a paper stencil, who are ignorant of the nature of paper when oiled; but I have been witness to Dr. Lind, of Windsor, printing from a single profile of the King, cut in paper, after he had used it for many thousand impressions, and saw that it was still unimpaired.

Tinfoil, or bismuth.

Many thousand impressions may be taken from an oiled paper stencil.

Again let us imagine thin and cheap pannels of wood dipped in fine plaster of Paris so as to coat both sides, and then smoothed and well hardened by oil: would not such a block afford good impressions, if we were to cut down its surface, and leave the relief in cameo? This I have tried in blocks of plaster of Paris, and made clear and good impressions from a drawing thus cut out of a small block; which, if not so hard as wood, is still hard enough to impress with, and, where few impressions only are wanted, a very good substitute among men who can make their own drawings, especially where they are objects that do not require effect.

Wood covered with plaster of Paris, and cut in relief.

I have also imagined, that for diagrams, or plans, very thick paper firmly glued to a smooth board would, when the interior of the design was cut away, give relief enough for printing, provided the lines were close enough; but in

Paper glued on wood and cut down.

printing

printing such things as this, we ought to apply the paper extended quite straight, and strike it down by a blow from a flat smooth instrument of the size of the block.

Pontipool ware.

On a Pontipool teaboard I can with a common stile or dry point at any time, with as much ease as I can draw, make an etching where not much force is required. By the same rule I can write on it, and use it as an engraved plate. The paper teaboards might perhaps even be found more useful.

Easy to engrave on pewter, or on copper slightly coated with white wax. Pencil drawings easily engraved thus.

On pewter, made very soft, it is easy to engrave, but very few people know, that on copper also it is as easy to draw with an iron stile, provided you prepare your ground of common white wax spread over it with a dabber when the plate is hot. This thin coat of white wax is useful to receive a pencil tracing of your drawing, which may be deposited on it by a slight friction applied to the back of the paper, or if large by the rolling press; and also serves effectually to prevent the dry point, or stile, from slipping when you begin to scratch the copper through the waxen coat. The burs you may take off by the common mode, or by charcoal partially if you want effect.

Incombustible ink on wood.

But to return from this not quite a digression, (for if we can learn to write backwards this dry point graving, through white wax, would be the best method of executing plates to imitate writing, and is I believe now used by writing engravers), let us consider cheapness of block as an absolute desideratum; and think whether, if we can find a material that resists the effect of flame, we may not write with that material, and char the rest a little way in, so as to leave a cameo letter—and if such a plan could be executed, poplar and beech might by machinery be soon reduced to level and cheap blocks. You will perhaps yourself smile when I tell you, that I have used blocks (if they may be so called), of pieces of carrot, to impress mathematical figures of solid forms for the purpose of facilitating instruction; and that a pack of cards were thus made by a son of mine on an emergency, using indian ink instead of oil colour.

Blocks of carrot.

A seal for prints or drawings, not easily imitated.

To stamp prints, drawings, or such sort of property, a volute shell ground down on a hone makes the securest seal in the world; for two cannot be made exactly alike. Sections of shells are indeed the best seals we can have, if we wish

wish to avoid imitation. Pearl shell and ivory are however too dear to bring them even into use as plates, and the same objection in a less degree lies against box wood. Glass, by Mr. Walker's method, decomposed by the Swedish acid may, we all know, be easily used as a substitute for both plates and blocks; but it must first be ground flat, and that expense puts an end to its utility. The soft turtle-shell might perhaps, when veneered on wood, be found to answer the purpose of printing—but this again will depend on its abundance. Silver also can be delicately destroyed by quick silver so as to make a block for printing, the form of the letters being first traced in varnish or thick oil; but this would only do for ciphers, or small heads or tokens, for obvious reasons. I have also thought, that, if we could write on a block of stone or plate of glass with an ink so thick as to leave the words in relief, we might, by pressing putty on it, take a cast sufficiently durable to make many impressions; or thus cast it in plaster of Paris from the relievo on the smooth block, but this would print the words white on a black ground; perhaps as good or better than black on white, as less dazzling.

Etching on glass.

Other materials.

Writing in relief.

There are other ideas, but these are enough I trust to rouse our thoughts on this important subject, and lest I tire you and your readers, I conclude by assuring you, that although I have not the honour to be known to you, I am always,

Yours, &c.

G. CUMBERLAND.

Bristol, Nov. 17, 1810.

VIII.

On the Classification of Chemical Agents. By MARSHALL HALL, *Esq. F. R. M. S. E. In a Letter from the Author.*

To Mr. NICHOLSON.

SIR,

IN your Journal for November, you have given insertion to a small communication from me, "On the Combinations of

of Oxigen;" I now transmit to you an application of the principle there proposed to the classification of chemical agents. I shall first present in order the arrangement, which I beg leave to suggest; and afterward, by way of elucidation, will add a few apposite and necessary observations.

Classification.

Classification of chemical agents.

Class I Attraction.
Class II Repulsion.

as producing a varied state of these.

1 Heat,
2 Light,
3 Chemical rays of the sun,
4 Electricity,
5 Galvanism, &c.

Class III Oxigen.
—— IV Aoxurets.

1 Simple combustibles,
2 Metals,
3 Metalloids.

Class V Oxurets.

1 Compound combustibles,
a Sulphur.
b Phosphorus.
c Azote?
d Hidrogen?
2 Acids,
3 Oxides,
4 Alkalis,
5 Earths,
6 Water.

Class VI Mineral,
—— VII Vegetable, and
—— VIII Animal compounds.

Study of a science to begin

Precise knowledge of the recondite phenomena of any science can only be attained by him, who has previously acquired

acquired its principles; the study of chemistry is therefore properly begun by an attentive consideration of those principles, by which its operations are produced and explained; and consequently that arrangement would appear to be the most proper, in which these are made to occupy the first place.

with its principles.

But I proceed to speak of the three classes, which are peculiar to the arrangement proposed. The first of these contains one substance only, namely oxigen. My reason for devoting to this matter one whole class is founded on its very extensive importance, and more especially on the relation which in this plan it bears to the two classes that follow. I believe there is no substance in nature, which is not capable of entering into combination with oxigen, except those substances which have already suffered a combination of this kind: such is the universality, and consequently such the high importance, of this chemical agent.

Classes peculiar to this arrangement.

Importance of oxigen,

This circumstance, added to the view of the relation of its combinations to each other, which I have partly given in my former communication, will, I trust, justify the plan, which I have adopted.

The fourth class comprehends those substances, which *do not* contain oxigen; the fifth comprises all substances, which *do* contain this matter. The articles of the fourth class do all combine with oxigen, and (as has been formerly stated) generally with each other: some of the individuals of the fifth class unite with a still farther quantity of oxigen, and, as in the last case, generally with each other. But no individual of the fourth class will enter into combination with any substance which is included under the fifth.

Substances in the 4th class do not combine with those in the 5th,

It has been said, that all the substances of the fourth class will combine with oxigen; they may therefore all be converted into articles of the fifth class; and hence it becomes possible, that any two substances whatever may, through the medium of oxigen, be made to combine.

except through the medium of oxigen.

I have alleged, that the substances of the fifth class will generally combine with each other; this however is not universally the case; for when this combination does take place, certain states, or degrees of oxidation, in the respective articles, are required. Many acids dissolve oxides in

Substances in the 5th class combine generally, but not universally, requiring a pa-

certain

culiar state of oxidation,

certain states of oxidation only. Thus if sulphate of iron be exposed in solution to the action of the atmosphere, the iron become farther oxidated, insoluble in the acid, and precipitates. Hence arises an extremely curious fact, namely, that a metallic oxide may precipitate a different oxide of the same metal. And on the other hand it sometimes happens, that an oxide will only combine with an acid when this latter contains a certain proportion of oxigen: this occurs in the mixture of the oximuriatic acid with the alkalis: a muriate and hyperoximuriate of the alkali are formed.

or disposing affinity.

Or if a combination of this kind should take place, an energetic disposing affinity is exerted, so that oxigen is attracted, and the appropriate states of oxidation come to be fulfilled; as is exemplified in the sulphurets, sulphites, phosphurets, &c.

New names.

With regard to the names, which I have suggested to express the articles of the fourth and fifth classes; I am by no means convinced of the necessity for their adoption, and less still am I satisfied with the denominations, which I have ventured to offer, especially with that of aoxuret; or, as it might be rendered, anoxuret. The term oxuret was chosen from its conformity with the general plan of chemical nomenclature, as in the examples, sulphuret, phosphuret, hidroguret, &c.; the privative particle *a* is prefixed to this term, to denote the substances of the fourth class.

Arrangement of substances as elementary or compund liable to perpetual change.

The plan, which arranges chemical substances according as they are elementary, or compound, is, from the daily advancement of chemical science, perpetually subject to change; for that substance, which we consider to day as simple, may very shortly be discovered to be compounded. The classification of substances, founded on relations in their chemical properties, since it is not liable to change from discoveries in the analysis of bodies, will be more permanent.

Advantages of the author's plan.

I imagine, that the arrangement, which I have suggested, will be allowed to possess, independantly of the advantages of the latter plan, certain others, which are peculiar to itself, and by no means unimportant. The assistance which it affords to the memory, and the extensive knowledge it

conveys

conveys of substances which admit of, or resist, mutual combination, are the advantages to which I allude.

I am, Sir,
Yours obediently,
MARSHALL HALL.

University of Edinburgh,
Dec. 13, 1810.

IX.

Remarks on Military Rockets; by Mr. HUME. *In a Letter from the Author.*

To Mr. NICHOLSON.

SIR,

THE description of the military rockets, detailed in the last number of your excellent Journal, affords me another opportunity of recognizing the schemes and proposals, which, above seven years ago, I offered to the Board of Ordnance. On this subject I have more to say than would be proper to intrude upon your pages, which ought not to be overcharged with controversial matter; but as this question is of considerable import to myself, for reasons I will not here explain, I trust to your indulgence to admit this letter into the Philosophical Journal. Improvements offered to the Board of Ordnance by the author,

In your correspondent's communication, there are some monopolizing assumptions, concerning these instruments, against which I must enter a decided protest; particularly as I feel my character as well as name implicated in the question in all its bearings, whether it concern the rockets, or, as they were originally named, the pyrotechnic *arrows*. and applicable to missile instruments in general.

The first of these sweeping conclusions is the following. Speaking of rockets generally, the author informs us "of their not-having been used until very recently as implements of warfare."

Now, to set this assertion at rest for ever, we have only to refer to the late wars in India, under Marquis Cornwallis; Rockets used in the wars of India.

lis; and, I should think, there were few officers on that service, who did not witness the employment of rockets as "implements of warfare." But this is not all, for in Major Dirom's "Narrative of the Campaign in India, 1792," we read of "a heavy fire of cannon, musquetry, "and *rockets*, opened from the works." The author adds, indeed, that "the fire from the garrison was *luckily* ill di-"rected." In other parts of the same volume, we have—"The enemy's line, on being approached, opened upon "them (our troops) with grape, musquetry, and rockets." —"The Sultan—sent a party of his people with rockets, to "disturb the camps during the night." In almost every chapter we may observe the military rockets are named, as well as rocket-boys, or those people, who had the chief management of this department. We are frequently reminded by the Major of the great clumsiness of their weapons, the bad discipline, the ill directed fire, and other analogous instances of the rude state of these natives, which show how far they are behind us in military science; we must, notwithstanding all this, acknowledge, that in the *use* of rockets as "implements of warfare" they have a prior claim to any man in this country.

Striking instance of this.

But I do not rest upon this single authority, although this is dated full nineteen years ago; for, in the 3d vol. of the "Asiatic Researches," there is the following paragraph, in an account of the battle of Paniput, on the 7th of January, 1761—"As the Rohillas had a great number of "rockets, they fired vollies of *two thousand at a time*, "which not only terrified the horses by their dreadful noise, "but did *so much execution also*, that the enemy (the Mah-"rattas) could not advance to the charge."

Hence, we must observe, the rockets were not appropriated to purposes of mere amusement and show, but positively as *implements of warfare, more* than *fifty years back* from the present day; for we ought not to suppose, that these instruments were all invented and constructed on the very day of the battle of Paniput.

Having thus got rid of one object, I shall now take leave to resist another; which, as it hinges upon the last inference, certainly stands upon no better foundation.

In

In a *note*, that it should not be entirely lost, we are informed that "The *invention*" of the military rocket is "exclusively due to one gentleman only."

The invention of military rockets therefore

After what has been advanced on Major Dirom's authority, and the cases quoted from the "Asiatic Researches," it would be superfluous to combat this grasping assertion. There is a most obvious distinction between an inventor and an *improver*; for improvement depends upon *others*, as well as on the genius and abilities of him who undertakes the management of the article *to be improved.* No man would say, that Mr. James Watt of Glasgow *invented* the steam engine, and leave the Marquis of Worcester's "Century of Inventions," the names of Savery, Newcomen, Crawley, and others out of the question. Nor, at the same moment, would any one withold from the worthy Mr. Watt all the admiration and gratitude so justly due to this incomparable mechanic, this wonderful, this towering genius, whose name in that particular branch can never be forgotten. Upon this part of my subject I, therefore, shall offer the following as the most legitimate inference; that there is no such person now living, as the *inventor of military rockets.*

not ascribable to any one now living.

The outlines of my claim to "*certain identities*", on which so much of the construction, perfection, and *efficacy* of modern rockets depend, are already before the public*; and I have never retracted a single syllable of that appeal. The *head* or *carcase* of the pyrotechnic-*arrow*, however my original design may have been tortured and garbled, was proposed by myself; and it is of little consequence to me, which end of the instrument is preferred to begin the combustion, as long as the effect eventually succeeds.

Claims of the author.

That many very important improvements have succeeded since Oct. 24, 1803, I am perfectly disposed to allow; I am not inclined to arrogate to myself the title of an *inventor* of military rockets, nor can I allow it, as I have said, to any other person whatever. Circumstantial evidence is often of the utmost value, and, when complete, is generally amply conclusive—in my case this species of proof is not

His suggestions improved upon.

* Gentleman's Magazine, Feb. 1808; and Times, Nov. 14, 1809.

 deficient.

A short statement of his case.

deficient. There were no *iron* rockets in use when I had the honour of being examined by Generals Congreve, Lloyd, Drummond (since dead, I believe,) and others, who formed that Committee of Colonels and Field-officers of the Royal Artillery. As the sketches and drawings, which I took with me to assist in description, were some years in the Royal Arsenal, it is no unreasonable question to ask, why my papers were not instantly sealed up, and, if not returned to me, carefully preserved from the inspection of those, who had no *official* concern with them. That these papers were *not* so preserved, there is no difficulty in proving; and I am compelled to say in the mildest terms, that the whole transaction bears the appearance of gross partiality and injustice against myself, as an humble individual, who stipulated for neither favour nor reward. I beg, once for all, to declare, that, of the gentlemen who composed the committee, speaking of them *individually*, I have no certain reason to complain; at the same time, I must consider the whole question open on all points for discussion, if it should ever be necessary.

I am perfectly aware of a "Reply*" to one of my letters; to this however I shall never make any *specific* rejoinder, till the name of its author is acknowledged by the editor of that newspaper; when I shall most willingly attend to all that this reply contains, as well as to many important circumstances, which for plain reasons its author has concealed.

Lest I be charged with asserting any of the above particulars, I shall close this subject by transcribing the last letter I received from the Board of Ordnance; and this is given to you rather as a voucher, that I have had some *connexion* with that department, than as a proof of the validity of my claim. This letter was in answer to my last request, "to have the papers and sketches returned with an official signature annexed." I should observe, however, that, when I first applied for them, the answer contained these words, on which I shall now make no comment.—"I am to observe, that I cannot discover those papers were ever received in my office". The following is a copy of the last letter, to which I have above alluded.

* Times, October, 16.

"SIR,

Office of Ordnance,
" SIR, *Oct.* 22, 1806.

" In compliance with the request, contained in your let-
" ter of the 14th instant, I herewith transmit the Sketches
" and Descriptions of your projectiles, which were submitted
" to a Committee of Colonels and Field-officers of Artillery,
" in October, 1803."

(Signed) R. H. CREW.

Trusting I shall not be teased with any more *anonymous* observations on this or any other attempt to vindicate my claim, I shall, for the present at least, quit this subject.

I remain, Sir,
Your obliged and obedient servant.
Long Acre, JOS. HUME.
December 17, 1810.

X.

Remarks on the Nature of the new Metals, Potassium and Sodium, in Answer to Mr. DALTON. *In a Letter from a Correspondent.*

To Mr. NICHOLSON.

SIR,

THE great ingenuity displayed in Mr. Dalton's new System of Chemistry, of which the second part has just appeared, must strike every one who reads it; and should his ideas prove hereafter to be really consistent with fact, the science will indeed have attained a "happy simplicity". It is not my intention to enter into a discussion of the general merits of Mr. Dalton's Theory, but I wish to call your attention, and, through the medium of your valuable Journal, that of the chemical world at large, to that part of the work alluded to, which treats of the nature of the bodies obtained from the fixed alkalis.

Mr. Dalton's theory of chemistry.

 Mr.

His reasons for considering potassium as a compound.

Mr. Dalton prefers the idea of potassium being a compound, rather than a simple substance, because, when potash, which has been previously fused (in which case, he says, it consists of 1 atom of water and 1 of potash) is submitted to the action of the Galvanic battery, it gives out no hidrogen: the effect of the electricity is to evolve oxigen at the positive pole, and this arises from the water of the hydrate—but the hidrogen (derived of course from the same source) draws with it an atom of potash to the negative pole, and they together constitute the compound atom potassium, consisting of potash 42 + hidrogen 1 = 43, and the residual potash is unaltered. In the French method, by the gun-barrel experiment, a quantity of hidrogen is evolved, and the results are, potassium, oxidated iron, and part of the potash employed. In this case, Mr. Dalton supposes (allowing however that the constitution of potassium here is not so obvious as when produced by electricity) that the hydrate is decomposed partly into potash and water, and partly into potassium and oxigen.

Remarks on the first.

In regard to the first conclusion, that no hidrogen is evolved, when *fused* potash is made the subject of experiment with the Voltaic battery, where is the *certainty* of the fact? At the high temperature requisite for the fusion of the potash, the potassium burns as soon as it is produced, and why not the hidrogen too? Atmospheric air is present, and we might almost as well infer, that no potassium is formed, as no hidrogen; if the proof is to rest on our inability, under these circumstances, of preserving it. We may, to be sure, catch, though with difficulty, a momentary glimpse of the one, which the nature of the other forbids. Could the decomposition of potash in fusion* be conducted in close vessels, so as to *collect* the gaseous products (and perhaps it might be done), the probability is, that *hidrogen would* be collected at the negative pole; and if so, all question as to the compound nature of potassium (at least as to its being formed

* The author here appears to have fallen into a mistake. Mr. Dalton is not speaking of the decomposition of potash while in a state of fusion; but of potash that has been previously fused, for the purpose of expelling from it water. C.

formed by the union of potash and hidrogen) would be done away with at once.

With respect to Mr. Dalton's second idea, that the hidrate of potash in the gunbarrel experiment seems to be decomposed partly into potash and *water*, and partly into pottassium and oxigen: what is the power or affinity, which produces this mixed decomposition? Mr. Dalton has told us, p. 470 and 471, that at a red heat potash remains in tranquil fusion—" but if the heat be increased, white " fumes begin to arise copiously. The *alkali and water* " *both evaporate* in this case—therefore the process cannot " be used to expel the last portion of water from the al- " kali."—It is not therefore according to himself the *temperature* of the " intensely heated iron tube," nor its contents, that separate the *water* from the potash. We are speaking of the water of course in its undecompounded state,—Mr. Dalton's words are, (as stated above) " into potash and water". Remarks on his second argument.

Mr. Dalton calls the potash used by the French Chemists in the foregoing method of decomposition, the *first hydrate*, meaning, I suppose, that which consists of 1 particle of water + 1 of potash, or, in *plain English*, potash containing 16 per cent of water, which is exactly the potash obtained by heating any other hydrate, containing more water, to redness, and which cannot, according to himself, be farther deprived of its water by heat. What then, I ask again, is the power, that produces this separation of *water* and potash? Iron heated to whiteness will *decompose* water, we know very well, but not that it will separate it, *undecomposed*, from potash. But how does Mr. Davy explain the fact? According to him, the affinity of iron for oxigen, and of iron for potassium, renders it capable of taking the oxigen both from the potash, and the water it contains; and in this it is assisted by the affinity of hidrogen for potassium, which at a high temperature is capable of dissolving it in large quantities, but deposits it again on cooling. This, in few words, is Mr. Davy's explanation of the decomposition of potash by the gunbarrel. The reader may find it more amply detailed in a note of that Gentleman to his Bakerian Lecture, in the Philosophical Transactions for 1809. Mr. Davy's explanation of the 2d case.

1809, p. 57*. Perhaps after having carefully perused that note, in which the following sentence occurs.—" Sodium appears to be almost *insoluble in hidrogen*, and " this seems to be one reason, why it *cannot be obtained*, " except in very minute quantities, in the experiment with " the gunbarrel."—The reader may be rather surprised on turning to p. 503 of Mr. Dalton's new system, to find the following.—" Though Mr. Davy's original method of obtaining sodium by Voltaic electricity is the most instructive, as to the nature of the new product, yet that of " Gay-Lussac and Thenard is the most *convenient*, when a " *quantity* of the article is required. *That is, to pass the* " *vapour of redhot hydrate of soda over iron turnings, in a* " *gunbarrel heated to whiteness*."—Mr. Dalton thus recommends a process for procuring sodium by *wholesale*, which Mr. Davy informs us will afford it only in very minute quantities. Which of the two is to be taken as authority in this case?

Another argument of Mr. Dalton from the lightness of potassium,

To follow Mr. Dalton a little farther in his arguments for the compound nature of potassium, he says, " The " levity of it, combined with its volatility at a low red " heat, agrees with the notion of its being potash and " hidrogen, or potassetted hidrogen, resembling the other " known compounds, of sulphur, phosphorus, charcoal, " arsenic, &c. combined with hidrogen." As to its low specific gravity being a proof in favour of Mr. Dalton's idea, it is answered by Mr. Davy in his paper " on the decomposition and composition of the fixed alkalis," p. 31 of the Philosophical Transactions for 1808†, in which (having stated that it was the opinion of most of the philosophical gentlemen, whom he had consulted, that the new substances should be called metals) he observes, that their *levity* is not sufficient reason for making them a new class, for there are remarkable differences in their respective specific gravities among the metals, as for instance, between platina and tellurium, the former being nearly four times heavier than the latter,—" and in the philosophical division " of the classes of bodies, the analogy between the greater

already answered by Mr. Davy.

* Journal, vol. XXIII, p. 256. † Ibid, vol. XX, p. 321.

" number

"number of properties must always be the foundation of "arrangement." Now I think Mr. Dalton will not deny this position; I would therefore ask him, whether the number of analogous properties is greatest between potassium and sodium, and the gaseous compounds of hidrogen, which he has enumerated, or between the *new* and the *common* metals. If the *new* metals be hydrurets, it follows, I think indisputably, that the common metals must be hydrurets; also and for that matter, the reasoning derived from their specific gravities makes as much against as for the argument, or rather it is decidedly adverse to it.

Mr. Dalton's remarks on the combustion of potassium in muriatic acid.

I have already exceeded the limits, to which I intended to have confined my remarks on this part of Mr. Dalton's new system; I shall therefore but briefly notice his observations on the combustion of potassium in muriatic acid. It is very singular, as he says, that the French chemists, and Mr. Davy, should have adopted the same explanation to account for the hidrogen produced, their views of the nature of potassium being so different. That, at the time he made the experiment, Mr. Davy should have concluded, that water contained in the gas was the source of the hidrogen, was perfectly natural; but, if Mr. Dalton be correct, water cannot exist, either in this gas, or in fluoric acid gas, (vide new System, page 282) and consequently Mr. Davy's conclusion must have been wrong; but Mr. Dalton's, and that of Gay-Lussac and Thenard, that it came from the potassium, are not more correct, for the late admirable "Researches on the oximuriatic acid" &c., as detailed in Mr. Davy's last paper, Phil. Trans. 1810, page 231 *, prove, that the true origin of the hidrogen was from the muriatic acid itself, independent of water. This acid being composed of the peculiar simple body, oximuriatic acid (I must call it by its old improper name, for want of a better), with a base of hidrogen; by the combustion of potassium in which a compound of oximuriatic acid and potassium is formed, and the base evolved.

Mr Davy's explanation of it.

The experiments prove muriatic acid

Mr. Dalton, in his appendix, mentions Mr. Davy's paper just alluded to, but still retains his opinion, that the hidro-

* Journal, vol. XXVII, p 321.

gen-

to be a compound of hidrogen and an acidifying substance.

gen results from the decomposition of the potassium. I cannot but think the facts prove absolutely the contrary, and that they are *supported* by the experiment of the combustion of equal parts of oximuriatic acid and hidrogen gasses, which gave muriatic acid for the result; the accuracy of which I can have no doubt of, although Mr. Dalton seems to hint, that implicit reliance on that head is not to be placed on it. Mr. Davy says it was attended with a condensation of $\frac{1}{10}$ or $\frac{1}{20}$ of the volume, and the deposition of a slight vapour ensued, which was always in smallest quantity when the gasses were driest. Mr. Dalton says, that he should have expected the condensation to have amounted to $\frac{1}{3}$ or $\frac{1}{4}$ on the common hypothesis; by which I understand, that he still considers oximuriatic gas as a compound of muriatic acid and oxigen. Now viewing it in this light, oximuriatic acid consists, according to Chenevix, of 77·5 muriatic acid + 22·5 oxigen by weight. In the combustion therefore of 100 measures of this gas, with an equal quantity of hidrogen, the oxigen (reckoning its specific gravity as 1·125, atmospheric air being 1·000) would take 60·48 measures of hidrogen, and a condensation of 90·72 would ensue, nearly half the whole quantity, and vapour in considerable abundance would be the consequence; but the slight portion mentioned by Mr. Davy resulted from another cause, as proved by its quantity diminishing, in proportion to the dryness and purity of the gasses employed. This immense difference between the condensation that does actually take place, and what ought to occur, according to the common hypothesis, is, I think, an incontrovertible argument in favour of Mr. Davy's conclusion, that muriatic acid is composed of a base of hidrogen united to a peculiar body, a substance *sui generis*, into which not a particle of oxigen enters, however similar the phenomena attending its union with many bodies may be to those produced by the combustion of inflammable substances in oxigen gas; since, as Mr. Davy justly remarks, the evolution of light and heat are only to be regarded as evidences of intense combination. With respect to the oxigen of the hyperoximuriate of potash, as it is called, the idea that it is furnished by the alkali seems to me to be the

the true one, which in this case is in the state of peroxide; but, in its common form (potash) it is only a protoxide.

"Potassium burns in fluoric acid gas, and the result is "fluate of potash and some hidrogen. The theory of this is not "obvious." According to Mr. Dalton's views it is not; but is it too bold a conjecture to imagine, that something similar to what takes place, when potassium is burnt in muriatic acid gas, occurs here also? and that hidrogen is probably the base of fluoric acid gas, united perhaps to some, as yet unknown body, of the same class as oximuriatic gas?

Fluoric acid perhaps a similar compound.

As the similarity between the properties of potassium and sodium is so great, I omit saying any thing on the latter, more than the remark already made, respecting the difference in the results with potash and soda in the gun-barrel experiment. The arguments for the undecompounded nature of potassium are equally applicable to that of sodium.

The same arguments apply to sodium as to potassium.

From a candid review of all the facts, I think it must be allowed, that Mr. Dalton does not make out his case; and that potassium and sodium consequently stand in the class, where their illustrious discoverer has placed them. It will be observed, that almost all the arguments, which I have brought forward in opposition to Mr. Dalton's idea of the compound nature of the new metals, are derived from Mr. Davy's papers: and it is not small praise, that he has sent his labours so perfect into the world (new and difficult as they have been), that his own conclusions, founded on and supported by his own experiments, form the ablest and most perfect defence against all the attempts, that have been made to overthrow them.

Mr Dalton's opinions respecting them therefore not proved.

JUSTUS.

XI.

Observations on the Diminution of Dilatability of Spirit of Wine Thermometers; by HONORE FLAUGERGUES*.

HALLEY and Muschenbroeck have asserted, that spirit of wine employed for thermometers in course of time loses

Spirit thermometers diminish

* Journal de Physique, vol. LXVI, p. 295.

nish in dilatability. loses part of its dilatability. Nollet and Brisson maintain the contrary, on the experience, as they say, of more than thirty years. The two following facts however confirm the opinions of Halley and Muschembroeck.

This shown in one made by Nollet, I have a spirit of wine thermometer, made in 1734 by Mr. Nollet, on the principles and under the inspection of Reaumur. From fear, no doubt, that the wire marking the freezing point might be displaced, Mr. Nollet wrote with his own hand on a paper pasted on the inside of the lid of the case of the thermometer the following note: "The upper extremity of the stem of the thermometer "ought to be at 94°:" and at this point he has made a groove. Hence it was easy for me to place it exactly in the same situation, as when it came out of the hands of that able philosopher. When I was thoroughly certain of this, I placed the thermometer in a vessel full of pounded ice, just melting; and in this state I kept it three days, adding fresh ice, when the former was half melted, and draining off the water. The liquor in the thermometer settled a degree and a quarter below nought, and remained stationary at this point during the whole time it remained in the ice. This experiment was made on the 7th of March, 1807, and the two following days.

and another by Romieu. I have another spirit thermometer made before 1758, according to Reaumur's principles, by Mr. Romieu, a learned natural philosopher of the Royal Society of Sciences at Montpellier, whose accuracy is generally known. This thermometer is enclosed and fixed in a large glass tube hermetically sealed, so that it cannot have been deranged. I placed this at the same time in a vessel of melting pounded ice, with which it was completely surrounded; and during the three days that this experiment continued, the spirit of wine continued stationary at two degrees and a quarter below nought.

Intended to be tried again at a future period. I preserve these thermometers, though grown very defective, in order to try whether their dilatability will diminish still farther. In the mean time these observations appear to me sufficient, to induce us to reject the use of spirit of wine thermometers; and keep to those of mercury, which do not appear liable to the same inconvenience.

REMARKS.

REMARKS.

In a subsequent number of the Journal de Physique Mr. Cotte observes, it is somewhat surprising, that the newest thermometer should have lost most: and he reminds Mr. Flaugergues, that in 1734 de Réaumur and Nollet fixed the freezing point by water just beginning to freeze, which he adds, according to Mr. De Luc's experiments, differs 0·8° from that of melting ice. By an oversight however he deducts these 0·8° from 1·25°, and thus reduces the loss of the thermometer to half a degree at most in 73 years. Correction of the preceding statement,

Mr. Flaugergues in reply admits the adjustment of the freezing point; and very properly subjoins, that, as he had placed it so much too low, the thermometer in reality did not rise so high, as when it was made, by 1·25° + 0·8°, or 2·05°, which amounts to nearly the same as the loss of Romieu's, the freezing point of which was no doubt that of melting ice. This therefore only tends to strengthen his observations; and he remarks, that two thermometers made by different persons, at very different periods, can hardly be expected to have contained spirit of precisely the same strength; which accounts for one having lost in dilatability rather more than the other in about three fourths of the time. which tends farther to confirm it.

It is to be regretted, that Mr. Flaugergues did not examine the dilatability of the spirit in higher temperatures also. C.

XII.

Analysis of the Amphibole of Cape de Gattes, in the Kingdom of Grenada: by Mr. LAUGIER*.

THIS amphibole is found among the volcanic products at Cape de Gattes. Its crystals have a black hue, and are formed Hornblende among the volcanic products

* Annales de Chimie, vol. LXVI, p. 325. Abridged from the Annales du Muséum d'Hist. Nat. by Mr. Bouillon-Lagrange.

of Cape de Gattes described.

formed of a multitude of laminæ, which give them a chatoyant appearance. Their fracture is transverse, and uneven. Spec. grav. 3·25. They scratch glass; and strike fire with steel, but not easily. Their primitive form and integrant particle are an oblique prism terminated by rhombs. Before the blowpipe they melt into a black glass. It breaks pretty readily; but is not easy to reduce to powder, more on account of the flexibility of its laminæ than its hardness. The coarse powder is of a dull green, the very fine powder of a greenish gray.

When exposed to a red heat it loses about 2 per cent, and retains on cooling a reddish hue from the oxide of iron contained in it.

Comparison between it and strahlstein.

The principal object of Mr. Laugier in analysing this substance was to compare the nature and proportions of the component parts of amphibole and actinote, between which crystallography finds a perfect resemblance. Though the results do not exhibit a resemblance so complete; Mr. Laugier does not think they differ so sensibly, but some resemblance may be found between the two stones. If the proportions of the substance that compose them be not exactly the same, at least we find in them the same principles*; and it appears no less established, that the differences found in them are not any of those that influence the form of the crystals. The absence of chrome and of an atom of potash in the amphibole; the presence of a larger portion of alumine, and a double quantity of iron in the same stone; and lastly, some other differences in the proportions of the remaining principles; do not essentially produce any very remarkable change in the crystallization, according to the observations of the most celebrated mineralogists†. It may not be useless to observe, that the difference between the proportions of the absolute quantities

* Not wholly. C.

† That is to say, whether a stone contain 11 per cent of oxide of iron, or above twice as much; about 10 per cent of magnesia, or almost 20; near 8 per cent of alumine, or not 1 per cent; 3 per cent of chrome, or not a single atom: the form of its crystals may be precisely the same, and they may exhibit no difference to their most expert dissectors. C.

of

of the principles of these two stones is much less considerable, than that which exists between these quantities themselves, when they are compared. Thus the result of this comparative analysis appears to bring actinote and amphibole so near together, that it seems necessary to include them in one species, as modern mineralogy has already indicated.

The following are the results of Mr. Laugier's analysis of amphibole, compared with those of actinote.

Component parts of the two.

	Amphibole.	Actinote.
Silex	42	50
Oxide of iron	22·69	11
Magnesia	10·90	19·25
Lime	9·80	9·75
Alumine	7·69	0·75
Oxide of manganese	1·15	0·50
Chrome		3
Potash		0·50
Water and loss	5·75	5·25
	100 *	100

SCIENTIFIC NEWS.

Royal Society of Edinburgh.

ON Monday, the 5th of November, the Royal Society of Edinburgh met for the first time in their new apartments in George street, when Dr. Thomas Thomson read two papers, giving an account of the analyses of two new minerals from Greenland. To one of these he has given the name of *allanite*, and to the other that of *sodalite*. In the first he discovered a considerable portion of cerium; and in one analysis he detected a quantity of a metallic oxide perfectly new in its

Two new minerals, one containing a new metallic oxide,

* These numbers are here as in the original; but the sum total would amount only to 99·98, so that there is an omission of 0·02 somewhere. C.

its properties, for which he proposed the name of junonium.

the other 0·23 of soda, and 0·3 of muriatic acid.

The other mineral according to his investigation affords 23 per cent of soda, and 3 of muriatic acid. By an analysis of Mr. Ekeberg, the same constituents were yielded in the proportions of 25 per cent, and 6 per cent.

Waterspout.

At the next meeting, on the 19th, a short communication was read respecting a singular waterspout, observed at Ramsgate.

Properties of the lever.

Mineralogy of Iceland.

On the 3d of December a paper by Dr. Brewster was read, being a new demonstration of the fundamental properties of the lever. Also a communication by sir George Mackenzie, bart., relative to the hot springs of Iceland, when sir George exhibited some beautiful drawings, and part of a series of magnificent specimens, from that country, which he proposes to deposit in the cabinet of the society; and at the last meeting, on the 17th, sir George began a description of the minerals of Iceland, when he exhibited specimens from the district called the Guldbringe Syssel.

New Agricultural and Commercial Magazine.

Mr. Clennell, of Homerton, whose institution of a Literary and Philosophical Society at Hackney we noticed a few months ago, will commence with the new year the publication of a "New Agricultural and Commercial Magazine; or general Depository of Art, Manufactures, and Commerce." It is obvious, that the subjects included under these heads form the basis of the comforts, enjoyments, and interests of individuals, and thence of society at large; and, from the names of those who have already engaged to contribute to the undertaking, much useful information and free and candid discussion may be expected in it. The term local has frequently operated as a bugbear, to prevent the communication of much valuable knowledge: but all our general information must originate from particular facts; and every fact is from its nature, in one sense of the word, local; while no one can be so strictly confined in its bearings and relations to this town, or that village, as to be incapable of furnishing useful hints or cautions to numberless others, if not to every one. Of this the conductor is fully aware, and

and accordingly solicits accurate and faithful information on all the subjects connected with his plan from every quarter.

Translation of Werner on Veins.

Werner's Theory of the Formation of Veins, with its Application to the Art of Working Mines, has been translated from the German, by Dr. Charles Anderson, of Edinburgh, member of the Wernerian Society, &c. It is accompanied with notes by the translator, some of which relate to mine districts in our island not noticed by Werner. It is unnecessary to say more of a work published about twenty years ago, than that it exhibits the results of nearly thirty years investigation by a man, whose experience and sagacity as a mineralogist have long been acknowledged.

Treatises on diseases of the eye.

Dr. Joseph Reade, of Cork, has just issuing from the press some Critical and Practical Observations on the Diseases of the inner Corner of the human Eye, comprising the epiphora, tumour of the lachrymal sac, and fistula lachrymalis, with a new arrangement and method of cure.

To Correspondents.

The communications from Mrs. Ibbetson, Mr. Dalton, Mr. Murray, and Mr. Bowditch, were too late for the present month, but will be inserted in the next number.

Meteorological

METEOROLOGICAL JOURNAL,

For DECEMBER, 1810,

Kept by ROBERT BANCKS, Mathematical Instrument Maker, in the STRAND, LONDON.

NOV. Day of	THERMOMETER. 9 A. M.	9 P. M.	Highest in the Day	Lowest in the Night.	BAROMETER, 9 A. M.	RAIN, noted at 9 A. M.	WEATHER. Day.	Night.
27	44°	45°	47°	38°	29·19		Cloudy	Cloudy*
28	42	45	47	35·5	·04	·095	Ditto	Rain
29	40	40·5	43	34	·02	·015	Rain	Fair
30	37	38·5	42·5	32	·18	·240	Fair	Ditto
DEC.								
1	35	37·5	39	30·5	·45		Ditto	Cloudy[a]
2	33	34	37	28	·92		Ditto[a]	Fair[a]
3	33·5	41·5	42	39	·95		Rain	Cloudy
4	42	47·5	48	42	·91	·090	Fair	Rain
5	46·5	48	49·5	45	·92	·025	Cloudy	Ditto
6	48	47	50	41·5	·60	·065	Rain	Fair
7	44	41	45·5	34	·33	·070	Fair	Ditto
8	37	36	41	30	·52		Rain	Ditto
9	32	34·5	36	30·5	·86	·015	Fair[a]	Ditto[a]
10	37	36·5	41	34	·55	·345	Rain	Cloudy
11	35	35	37	29·5	·60	·250	Fair	Fair
12	35·5	44	49	40	·73		Rain	Ditto
13	47	50·5	51	46	·82	·120	Cloudy	Rain†
14	50·5‡	45	50·5	38	·63	·210	Rain	Fair
15	42·5	44·5	45·5	37	·86	·170	Fair	Ditto
16	41	39	42	38	30·22		Fog	Ditto[a]
17	41	47	48	44	·28		Cloudy	Cloudy*
18	48	41·5	48·5	38	29·63	·030	Rain	Fair
19	41	42	42	34	·47	·330	Fair	Ditto[a]
20	37	45	48	37	·85		Rain	Ditto
21	44	43	48	38	·33	·110	Ditto	Fair
22	42	50	52	46	·62	·040	Ditto	Rain
23	48	45	53	42	·40	·260	Ditto	Fair
24	44	42	50	38.5	·31	·350	Ditto	Ditto
25	48	46	49·5	41	·31	·200	Ditto§	Cloudy
26	45	45·5	51	41	·58	·285	Fair	Ditto
						3·315 Inch.		

* Rain in the night. [a] Intervening fogs. † Stormy night. ‡ Maximum of Heat at 9 A. M. § Stormy morning, hail at half past 4 P. M. lightning in the evening, and stormy night.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

FEBRUARY, 1811.

ARTICLE I.

Inquiries concerning the Signification of the word Particle, as used by modern Chemical Writers, as well as concerning some other Terms and Phrases. By Mr. JOHN DALTON. *In a Letter from the Author.*

To Mr. NICHOLSON.

SIR.

IN perusing the modern books, which are edited under the titles of Elements or Systems of Chemistry, I have been greatly perplexed in endeavouring to understand the meaning, which the authors attach to certain words and phrases, which are of perpetual occurrence, particularly when treating of *affinity*, but which the authors no where define. I allude more especially to the word *particle*. Meaning of chemical terms undefined.

With respect to the article *affinity* itself it is somewhat remarkable, that different authors should view it in such different lights. Lavoisier in his Elements of Chemistry does not say a word on the subject, except a few observations in his preface tending to show the difficulties of it, and its unfitness to be presented to a novice in chemistry. In Affinity.

speaking of the number and nature of elements, he says, "I shall therefore only add on this subject, that, if by the term *elements* we mean to express those simple and indivisible atoms of which matter is composed, it is extremely probable we know nothing at all about them." Chaptal employs about 20 pages on affinity, two on the affinity of aggregation, and the rest on the affinity of composition. In the third edition of your First Principles of Chemistry, I observe 10 pages on affinity. Berthollet has written a small volume on the subject. Fourcroy, Thomson, Murray, and Henry, have all expanded their views on this subject pretty extensively. From these observations it should seem, that the article *affinity* in systems of chemistry has been of *growing* importance since the time of Lavoisier's publication. This may certainly be right; but as the subject is acknowledged to be one of the most difficult in the science, it is highly necessary, that authors should treat it with all the perspicuity of which it is capable; the terms used should be clear and well defined; metaphorical expressions should be avoided; and ambiguities should be guarded against with all possible care.

Chaptal. Chaptal has been very sparing in the use of the term *particle* when treating of affinity (I refer to your translation, 2d ed.); he only mentions it three times, twice unqualified, and once along with the epithet *elementary*; he seems to use *integrant part*, where others would use particle, and he defines it thus: "two drops of water, which unite together into one, form an aggregate, of which each drop is known by the name of an integrant part."

Integrant part.

Nicholson's First Principles. On the subject of affinity I observe you use the term *particles* frequently, but only once in the singular number. No definition of it is ever given; but the last time it occurs is in a very judicious concluding remark, which I do not remember to have seen in any subsequent writer; namely, "it seems reasonable to infer, that two compounded particles coming together by attraction, undisturbed by any other cause, should dispose themselves so as to apply such sides of each together, as are occupied by principles the most attractive of each other." In another part of the chapter you observe, that "the minutest parts, into which an

an aggregate can be *imagined to be* divided without decomposition, are called integrant parts; but the parts into which it is divided by decomposition are called component parts or principles." These definitions would be clear and intelligible to me, if the words I have put in italics were omitted*; the sense affixed to *integrant parts* is better than that of Chaptal, and is the same as the most correct modern writers apply to *integrant particles*. But, after thus defining the terms *integrant parts*, I do not find, that you have again used them throughout the chapter. Integrant and component parts.

From the high encomiums, which several modern writers pass on Berthollet's researches into the laws of chemical affinity, I was induced to hope, that in a volume written exclusively upon affinity by so able a hand, I should find the term *particle* clearly defined in the first page. I took up the English translation, and read 37 pages without once meeting with the word *particle*, or any other word of similar import. I was beginning to think, that the author intended to abolish the old doctrine of bodies consisting of extremely small parts bound together by a principle of cohesion, or affinity of aggregation, and to establish the one that bodies consist entirely of cohesion, when in the opening of the 5th article I read, "the cohesion of the moleculæ of a body is due to the reciprocal affinity of these moleculæ;" the word moleculæ did not again occur in the volume to my cursory observation. In the same page I met with the phrase *integral parts*, and afterward in the space of 150 pages the words *part* and *particle* were observed in about a dozen places. Whatever obscurity may be found in Berthollet's researches then, they do not arise from the frequent use of the term *particle*. Berthollet on chemical affinity. Moleculæ.

* I do not know how Mr Dalton would understand the passage, if the words in italics were omitted; but they appear necessary, to express the true meaning with precision. Suppose a single atom of sulphuric acid combines with a single atom of mineral alkali to form a particle of sulphate of soda, and a crystal to be formed by the aggregation of such particles: we cannot actually divide it into these component parts, though we can imagine it so divided. The word *imagined* may be here taken as synonimous with the word *conceived* of the geometers, and has no necessary relation to operative or human practicability. N.

Dr Thomson on affinity.

Dr. Thomson, in a short essay on affinity in general, uses the term *particle* frequently, and without any direct definition; but as he advances his idea of *particle* is gradually developed. Indeed he is almost the only writer, who has courage to talk of *a particle, two particles,* &c. *Homogeneous* particles are those of the same body; *heterogeneous* particles those of different bodies; thus "a particle of iron and a particle of lead are heterogeneous." Again, "an integrant particle of water is composed of particles of hidrogen and oxigen urged towards each other, and kept at an insensible distance, by heterogeneous affinity; and a mass of water is composed of an indefinite number of integrant particles of that fluid, urged towards each other by homogeneous affinity." He supposes the particles of matter to have figure and magnitude, and adds, "if the particles of bodies have length, breadth, and thickness, we cannot avoid conceiving them as composed of an indeterminate number of still more minute particles or atoms. Now the affinity of two integrant particles for each other must be the sum of the attractions of all the atoms in each of these particles for all the atoms in the other," &c. Here the meaning of a *particle* is very clearly pointed out. Bodies, such as iron, are constituted of *particles* connected together by homogeneous affinity; these particles are each of them constituted of a certain unknown number of subordinate particles, which last are the atoms or ultimate particles, into which the body is capable of being resolved without decomposition. This notion is clear as far as it goes (I mean not as to its truth, but as to the expression); it would seem however to require something to be said about the *cohesion* of the atoms to form a particle, namely, whether it is the same or different from the cohesion of the particles to form a mass; in other words, whether the force of cohesion, which binds together the atoms or first order of particles, is the same as that which binds the second order of particles. See Thomson's Chemistry, 3d ed. vol. iii.

Murray's Chemistry.

Mr. Murray uses the term *particles* frequently; he commences his work with the observation, that "all the phenomena of chemistry arise from the attractions and repulsions exerted between the particles of matter;" but I have not been

been so fortunate as to meet with his definition of the unqualified term *particle.* He has however given the best definition I have yet seen of *integrant particles.* "The calcareous spar, to take it as an example, may be reduced to a particle, beyond which the division cannot be carried without resolving it into its elements, lime and carbonic acid; or, at least, it may be reduced to a particle, beyond which, if its minuteness allowed us to operate upon it, it is demonstrable its figure would not change. To these last particles, the result of the mechanical analysis, Haüy gives the name of *integrant particles*, and their union constitutes the crystal." See Chemistry, 1st edit. vol. i, page 56. Again, on *integrant* and *constituent* particles, "the constituent or component parts are substances differing in their nature from each other, and from the substances they form. The integrant parts are precisely similar to each other, and to the general mass, which is composed by their union; or they are *merely* the smallest particles into which a substance can be resolved without decomposition; while decomposition is always implied in the division of a body into its constituent particles. The integrant parts are united by the force of aggregation, the constituent parts by chemical affinity," &c. All this is more than perspicuous; it is excellent; and from the familiar manner in which Mr. Murray introduces "the smallest particles, into which a substance can be resolved without decomposition," and from the precise definition of them, one would be inclined to believe the author was going to say something more about them in the sequel; but he scarcely mentions them again, while the unqualified term *particles* is perpetually introduced.

Dr. Henry adopts the common distinction of the attraction of aggregation, and that of composition, or affinity. "In simple bodies, therefore, the cohesive affinity alone is exerted; but compounds are influenced by both affinities, their *constituent* or dissimilar parts being united by chemical affinity, and their *integrant* or similar parts by the affinity of aggregation." Chemistry, 6th ed. vol. i, page 52. He does not however clearly define *integrant parts;* it is not sufficient to say, that "a lump of copper may be considered as

Henry's Chemistry.

as composed of an infinite number of minute particles or integrant parts, each of which has precisely the same properties as those that belong to the whole mass." For, it is obvious, such *integrant parts* may either be Dr. Thomson's *particles* (of the first order) or Mr. Murray's smallest particles into which a substance can be resolved without decomposition, which I call atoms. Dr. Henry uses the term *particles* at times in the same indefinite manner as his predecessors.

Words improperly used.

Ratio.

There are some other current expressions among chemical writers on affinity, that are either erroneous or misunderstood. Chaptal says, that "the affinity of composition is in the inverse ratio of the affinity of aggregation." Dr. Henry has adopted a similar expression, page 53. The only thing that can be intended by this expression is, that the affinity of composition is less efficacious, as the affinity of aggregation is greater; but not the affinity has precisely *half* the effect, when that of aggregation is *double*.

Berthollet on the effect of quantity on affinity,

Some new opinions and expressions were introduced by Berthollet, which want both confirmation and illustration; one of them is, that the effect of affinity is in proportion to the weight of a body multiplied by the degree of its affinity. Mr. Murray, page 85, has it: "the chemical action of any body is exerted in the ratio of its affinity and quantity." Dr. Henry observes, page 72, "to obtain a measure of the action of two bodies on a third, if their respective affinities were precisely determined, it would only be necessary to multiply the number indicating the affinity by the quantity." Berthollet's language on this subject would seem to warrant this conclusion as his; but the experiments he adduces certainly warrant no more than that the effect "is modified considerably by the mass," as Dr. Thomson expresses it. Mr. Murray however has attempted to illustrate the *truth* of the principle (Appendix, page 19), and Dr. Henry to explain its *meaning* (page 72), according to the sense in which they understand it. The latter has succeeded very well; but the former, having a much more difficult task, has, I think, still left some obscurity about it. The explanation is this: "if a particle of sulphate of barytes be on every side in contact with particles of potassia,

what

what additional force can be gained from an additional quantity of potassia, which must be without the sphere of action?" This is a very proper statement of the question. I wish the author had kept closely to it, and not substituted for it another and more complex one. Mr. M. continues; "this objection however may be obviated, by considering the manner in which quantity operates. Suppose two bodies, A and B, to be presented to each other, the particles of each within the requisite distance will combine together in that proportion, in which they will be mutually saturated. Suppose it to be the one which is in larger proportion; its mass will continue to operate, and, even if there be no successive application of it to B, from the motion attending the combination, its uncombined or unsaturated particles will take *part of the particles* of B which have entered into the combination; so that the *whole particles* of B which have been attracted by A, instead of remaining combined with part of it, in the proportion in which saturation takes place, will be diffused through the mass; and hence by an increase in this mass there will be an addition to the force of affinity." Mr. M. then adds, "no law with regard to chemical affinity is more important than that now illustrated." But the illustration is almost lost upon me, for want of knowing the precise meaning of *particle*.

Another of Berthollet's observations is, "that the action of a substance, which tends to decompose a combination, diminishes in proportion as its saturation advances; for this substance may in such case be considered as composed of two parts, one of which is saturated, and the other free. The former may be considered as inert, and as unconnected with the latter, the quantity of which diminishes according as the saturation advances; while on the contrary the action of that which has been eliminated increases in proportion to the augmentation of its quantity, until the equilibrium of the contending forces ends the operation, and limits the effect." Researches, page 15. I apprehend it is to this that Mr. Murray alludes, page 82, when he deduces the law, "that the force of attraction is in the inverse ratio of saturation, or that the particles which enter first into combination are retained by a stronger affinity than those afterward

and on that of the progress of saturation.

ward combined;" and Dr. Henry, page 73, when he says, "the force of affinity is in the inverse proportion of neutralization." This principle Dr. H. explains numerically by supposing 100 parts of potash to be divided into portions, and successively put into sulphuric acid, the first portion, he argues, will be held more forcibly than the second, &c. Now whatever errours Mr. Berthollet may have fallen into, I think he cannot be fairly charged with this; his commentators here appear to me to be responsible. Suppose, instead of potash, barytes is substituted; would these gentlemen say, Berthollet maintains the first formed sulphate of barytes would be with more difficulty decomposed than the last formed? I think no such inference can be fairly drawn from his position. If I understand Berthollet, he means, that, if a combination, as sulphate of potash, have another substance put to it, tending to decompose the combination, as barytes, the action of the barytes will be less and less powerful as the decomposition goes forward; the great quantity of potash liberated will lend its assistance to that still in combination, and both finally will prevent the small quantity of barytes remaining from effecting a complete decomposition of the sulphate of potash.

Here mistaken by his commentators.

His meaning explained.

I would not have it to be understood, that by these inquiries I wish to depreciate your publication on chemistry, or those of the ingenious gentlemen whom I have had occasion to mention. The works have their excellencies; and if they should also have their defects, it is of interest to the public and to the authors to be made acquainted with them. I hope either you, or some of your readers, will undertake to explain what you conceive chemical authors signify by *particles;* and then I think we shall be enabled soon to dispel the mists, that envelope the fundamental principles and operations of the science.

I am yours,

J. DALTON.

Manchester, Dec. 19*th.*

II.

II.

An Estimate of the Height, Direction, Velocity, and Magnitude of the Meteor, that exploded over Weston, in Connecticut, December the 14th, 1807: with Methods of calculating Observations made on such Bodies. By NATHANIEL BOWDITCH, *A.M. A.A.S. and Member of the Philosophical Society held at Philadelphia.**

Meteor that exploded over Weston, in Connecticut.

THE extraordinary meteor, which appeared at Weston, in Connecticut, on the 14th of December, 1807, and exploded with several discharges of stones, having excited great attention throughout the United States; and being one of those phenomena, of which few exact observations are to be found in the history of physical science; I have thought, that a collection of the best observations of its appearance at different places, with the necessary deductions for determining, as accurately as possible, the height, direction, velocity, and magnitude of the body, would not be unacceptable to the Academy; since facts of this kind, beside being objects of great curiosity, may be useful in the investigation of the origin and nature of these meteors; and as the methods of making these calculations are not fully explained in any treatise of trigonometry common in this country, I have given the solutions of two of the most necessary problems, with examples calculated at full length. The second problem is not (to my knowledge) given in any treatise of spherics. The observations of the meteor, which, after many inquiries, were found to have been made with sufficient accuracy to be introduced in the present investigation, were those made at Wenham, about seven miles northeasterly of Salem, by Mrs. Gardner, a very intelligent lady, who had an opportunity of observing it with great attention; those at Weston by Judge Wheeler and Mr. Staples; and those at Rutland in Vermont by William Page, Esq.

Observations of it.

* Memoirs of the American Academy of Arts and Sciences, Vol. III, Part II, p 213. Communicated by the author.

In

In collecting and combining these observations I have received great assistance from my friend, John Pickering, jun. Esq. A.A.S., particularly in the observations at Wenham. These observations are given after the problems, and the results from combining them in various manners are stated at the end of this memoir.

Problem 1. *Plate* III. *Fig.* 1.

Methods of determining the place of a meteor.

Suppose that in two places in given latitudes and longitudes the azimuths of a meteor were observed at the same moment of time, and its angular elevation above the horizon of one of those places. It is required to determine the situation of the meteor.

Solution.

Let C be the centre of the Earth, PWSM a portion of its surface reduced to the level of the sea supposed to be spherical, P the pole of the Earth, *w*, *s*, the places of observation, *m* the place of the meteor. Draw C*w*, C*s*, C*m*, cutting the sperical surface in the points W, S, M. Then W*w*, S*s*, will represent the vertical heights of the places of observation, and M*m* the vertical height of the meteor above the level of the sea; PWM the azimuth of the meteor observed at *w*, and PSM its azimuth observed at *s*. Then in the spherical triangle PWS are given PW, PS, the colatitudes of the places of observation, and the angle WPS equal to their difference of longitude, to find by spherics the angles PWS, PSW, and the side SW. The sum or difference of PWS, PWM, will give the angle SWM; and the sum or difference of PSM, PSW, will give the angle WSM. Then in the spheric triangle WSM will be given the angles SWM, WSM, and the side SW, to find the sides WM, SM, which are respectively equal to the angles *m*C*w*, *s*C*m*. The altitude of the meteor observed at *w*, added to 90°, and $\frac{1}{14}$th part of the arch WM subtracted from the sum for terrestrial refraction, will leave the correct value of the angle C*wm*: this added to *m*C*w*, and the sum subtracted from 180°, will leave the angle C*mw*. Then in the plane triangle C*wm*, will be given the angles and the side C*w*, (which may in general, when W*w* is small, be taken equal to

to the semidiameter of the Earth, 3982 miles*) to find *wm* the distance of the meteor from the observer at *w*, and C*m* its distance from the centre of the Earth, from which subtracting CM equal to 3982 miles, there will remain the vertical altitude of the meteor above the level of the sea. In the plane triangle *s*C*m* are given C*s*, C*m*, and the included angle *s*C*m*, (= arch SM) to find *sm* the distance of the meteor from the observer at *s*, and the angle C*sm* equal to the supplement of the zenith distance of the meteor at *s*. The colatitude of the meteor is equal to the arch PM, and the angle SPM is equal to the difference of meridians between the meteor and the observer at *s*. These quantities may be easily found, by means of the spheric triangle PSM, in which PS, SM and the angle PSM are given. They may also be found in a more simple manner, and to a sufficient degree of accuracy, by the usual rules of navigation, supposing the angle PSM to be the course and SM the distance, whence may be found the difference of latitude, departure, and difference of longitude between the points S, M.

Example.

Suppose the azimuth of the meteor at Wenham PWM= 117° 35′ 54″, azimuth at Weston PSM=3°, altitude at Wenham 5° 50′ 40″, colatitude of Wenham PW=47° 19′ 45″, colatitude of Weston PS 48° 45′, difference of meridians WPS=2° 36′ 45″. It is required to find the latitude and longitude of the meteor, its distance from Wenham and Weston, and its vertical height above the level of the sea. The first problem exemplified.

This corresponds to Example 10 in Table 1.

* The mile made use of in this memoir is the statute mile of 5280 feet. In the following calculations on the Weston meteor, it will be supposed that C*w*=C*s*=3982 miles; the part W*w* or S*s* being but a small fraction of a mile.

In

In the triangle PSW.

$\frac{1}{2}$PS = 24°22′30″
$\frac{1}{2}$PW = 23 39 52

Sum	48 2 22	Cos. AC	0·1748214	- - - - - Sine AC	0·1286575	PSW	52°56′34″	Sine AC	0·0979786
Diff.	0 42 38	Cos.	9·9999666	- - - - - Sine	8·0934643	PW	47 19 45	Sine	9·8664408
$\frac{1}{2}$ WPS	1 18 22	Cotan.	11 6420673	- - - - - Cot.	11·6420673	WPS	2 36 45	Sine	8·6587852
$\frac{1}{2}$ Sum	89 37 36	Tang.	11 8168553	$\frac{1}{2}$ diff 36°11′2″ Tan	9·8641891	SW	2 24 25	Sine	8·6232026
$\frac{1}{2}$ Diff.	36 11 2								

Sum	125 18 38=PWS	
Diff.	52 56 34=PSW	

PWS	125 18 38
PWM	117 35 54
SWM	7 42 44

PSW	52 56 34
Azim. PSM	3
WSM	55 56 34

In the triangle WSM.

$\frac{1}{2}$WSM 27 58 17
$\frac{1}{2}$SWM 3 51 22

Sum	31 49 39	Cos. AC	0·0707652		Sine AC	0·2778898
Diff.	24 6 55	Cos.	9·9603401		Sine	9·6112706
$\frac{1}{2}$SW	1 12 12$\frac{1}{2}$	Tang	8·3223773		Tang	8·3223773
$\frac{1}{2}$Sum	1 17 34	Tang.	8·3534826	$\frac{1}{2}$dif. 0°55′57″	Tang.	8·2115377
$\frac{1}{2}$Diff.	0 55 57					

Sum 2 13 31=WM=mCw
Diff. 0 21 37=SM=sCm

Cwm=95°41′50″=Alt. 5°50′40″+90°−$\frac{1}{12}$WM*
- - - - - - 2 13 31
Cmw=82 4 39

Sum 180 0 0

* The correction $\frac{1}{12}$WM, by the above calculation, is 9′32″. The value made use of in finding Cwm is 8′50″, estimated by a rough calculation, and it was thought unnecessary to repeat the operation on account of this small difference.

In

In the triangle Cwm.

Cmw	82° 4′ 39″	Sine AC	0·0041651	- - - - - - - - - - -			0·00417
Cw	3982	log.	3·6001013	- - - - - - - - - - -			3·60010
Cwm	95 41 50	Sine	9·9978494	WM	2° 13′ 31″	Sine	8·58915
Cm	4000·51	log.	3·6021158	*wm*	156·1 miles	log.	2·19342

In the triangle Csm.

Cm+*Cs*	7982·51	log. AC	6·0978605	*Cms*	53°24′36″	Sine AC	0·09533
Cm∽*Cs*	18·51	log.	1·2674064	*Cs*	3982	log.	3·60010
½sum ang.	89 49 11½	Tang.	12·5025138	*sCm*	0 21 37	Sine	7·79851
½diff. ang.	36 24 35½	Tang.	9·8677807	*sm*		31·2 miles log.	1·49394

Sum 126 13 47=*Csm* ∴ Alt. at Weston 36° 13′ 47″.
Diff. 53 24 36=*Cms*

The course from S to M is nearly N 3° W, the distance SM 21′ 37″, or 21 62′; hence the difference of latitude is 21·59′=21′ 35″, the departure 1·13′, and the difference of longitude 1·51′=1′ 31″

Latitude of Weston	41° 15′ 0″	Longitude of Weston	73° 27′ 0″
Diff. of latitude	21 35	Diff. of longitude	1 31
Latitude of Meteor	41 36 35N	Long. of Meteor	73 28 31 W.

Problem 2, *Fig.* 2.

Problem 2. Suppose that in two places, s, w, in given latitudes and longitudes, the angular elevations of a meteor Csm, Cwm, were observed, and the azimuth PSM at one of the places. It is required to find the situation of the meteor.

Solution.

This figure is to be marked like the first, then on SM (continued if necessary) let fall the perpendicular AW. Suppose a plane drawn through w, perpendicular to Cw, to cut the line sm in b. Join Cb cutting SM in B, and let CA continued cut sm in a. Find in the triangle PSW, the angles PWS, PSW, WSM (or ASW), and the side SW as in the laſt problem. Then in the right-angled spheric triangle SAW are given SW and the angle ASW, to find by spherics SA, AW, and the angle SWA. The angle Csm, or its supplement, is equal to the angle Csa, the angle sCa = arch SA, and the angle $Cas = 180° - Csa - sCa$.

$$\text{Sine } Cas : \text{Sine } Csa :: Cs : Ca$$

$$\text{Tang. AWB} = \frac{\text{Tang. } Cas}{\text{Sine AW}} \left(1 - \frac{Ca}{Cw} \cdot \text{Cosine AW}\right)$$

The affection of the angle AWB may be determined by the figure and the data of the problem.*

$$\text{Sine MWB} = \frac{Ca}{Cw} \times \text{Cotang. } Cwm \times \text{Tang. } Cas \times \text{Cos. AWB}.$$

$$\text{MWA} = \text{AWB} \underset{\backsim}{+} \text{MWB}.$$

The sign to be made use of is easily discovered by the figure, observing that the point B falls between M and S.

Cotang. mCw (= Cotang. MW) = Cosine MWA × Cotang. AW. The sum of the angles Cwm, mCw, subtracted from 180°, leaves Cmw. Then

$$\text{Sine } Cmw : \text{Sine } Cwm :: Cw : Cm.$$

The distances wm, sm, and the latitude and longitude of the meteor may be found as in Problem I.

Correction for refraction. When the diſtance of the meteor is great, the angles Csm, Cwm,

* This angle may also be found in the following manner. Having in the plane triangle aCw, the sides Ca, Cw, and the angle aCw (= arch AW) the angles Caw, Cwa, may be found by plane trigonometry, and AWB by this formula. $\text{Tang AWB} = \frac{\text{Cos } Cwa \times \text{tang } Cas}{\text{Sine } Caw}$

Cwm, muſt be corrected for terreſtrial refraction, by ſubtracting one fourteenth part of the intercepted arches SM, WM reſpectively; but as theſe arches are generally unknown, it will be neceſſary (when great accuracy is required) to make the calculation with the altitudes uncorrected, and thus find the approximate values of the corrections of the altitudes for refraction, and, by repeating the operation, the required quantities will be obtained. In this way the refraction in the Wenham obſervations was found to be about nine or ten minutes.

Example.

Suppoſe the altitude of the meteor at the time of its diſappearance at Weſton was 75°, its azimuth at that place N15°W, and the correſponding altitude of the meteor at Wenham 5°30′ corrected for refraction. It is required to find the latitude and longitude of the meteor, its diſtance from Wenham and Weſton, and its vertical altitude above the level of the ſea. Exemplification of prob. 2.

Here are given the angle $Cwm = 90° +$ alt. at Wenham $=$ 95° 30′, $Csm = 90° +$ alt. at Weſton $= 165°$. The arch SW $= 2° 24' 25''$, the angle PWS $= 125° 18' 38''$, and the angle PSW $= 52° 56' 34''$, are found as in the laſt example. The angles PSW, PSM, added together, give WSM or ASW $= 67° 56' 34''$.

This correſponds to Example 14, Table 1.

SW

SW	2°24′25″	Tang.	8·6235990
ASW	67 50 34	Cos.	9·5746474
SA	0 54 16	Tang.	8·1982464
Csa	165 0 0	Sine	9·4129962
Cas	14 5 44	Sin. AC	0·6134301
Cs	3982	log.*	
Cw	3982	log. AC*	
Ca ÷ Cw		log.	0·0264263
AW	2 13 50	Cos.	9·9996708
	1·061933	log.	0·0260971
Sub. 1	·061933	log.	8·7919221
Cas	14 5 44	Tang.	9·3998470
AW	2 13 50	sin. AC	1·4097939
AWB	21 46 44	Tang.	9·6015630
AWB	21 46 44	Cos.	9·9678392
Ca ÷ Cw		log.	0·0264263
Cwm	95 30	Cot.	8·9835769
Cas	14 5 44	Tang.	9·3998470
MWB	1 22 2	Sine	8·3776894
AWB	21 45 44		
MWA	20 24 42		
SWA	22 4 29		
SWM	1 39 47		
PWS	125 18 38		

PWM 123 38 51 = calculated azimuth.

- - -		Sine	8·6232157
- - -		Sine	9·9669904
AW	2°13′50″	Sine	8·5902061
AW	2 13 50	Cot.	11·4094901
MWA	20 24 42	Cos.	9·9718374
MW	2 22 47	Cot.	11·3813275
Cwm	95 30 0	Sine	9·9979960
Cmw	82 7 13	sin. AC	·0041201
Sum	180 0 0		
Cw	3982	log.	3·6001013
Cm	4001·5	log.	3·6022174
Mm	19·5		
MW	2°22′47″	Sine	8·6182788
MWA	20 24 42	Sine	9·5425303
AM	0 49 47	Sine	8·1608091
SA	0 54 16		
SM	0 4 29	Sine	7·11533
Csm	165 0 0		
Cms	14 55 31	Sine AC	0·58912
sum	180 0 0		
Cs	3982	log.	3·60010
sm	20·2	log.	1·30455

- - -		Cos.	9·9996167
- - -		Tan.	0·3923430
SWA	22°4′29″	cot.	0·3919597
- - -		Sine	8·61823
- - -			0·00412
- - -			3·60010
wm	166·9	log.	2·22250

The course from S to M is nearly N 15° W, the distance SM 4′ 29″; hence the difference of latitude of the points S, M is 4′ 20″, departure 69·6″, difference of longitude 93″. Hence the latitude of the point M is nearly 41° 19′ 20″ N, and its longitude 73° 28′ 33″. W.

If it be required to find the change in the above elements arising from an errour in the altitude at Wenham, it would only be necessary to repeat the latter part of the calculation, since the values of SA, SWA, AWB, would remain the same in both cases.

Remarks. When the distances between the observers of the meteors are small.

Remark 1. When the distances of the observers from each other and from the meteor are small, the correction arising from the spherical form of the Earth may be neglected, supposing the triangle SWM to be rectilinear, and drawn on a horizontal plane. In this case the calculations will

* These logarithms are neglected, because one is the arithmetical complement of the other, and their sum (rejecting 10 in the index) is 0

will be rendered more simple, if the heights are estimated from a plane drawn through w parallel to the horizon, supposing the points w, W, to coincide. Then if the points, s, w, are at the same height, the points s, S, will also coincide as in fig. 3. If the points s, w, are not at the same level, as in fig. 4, the lines ms, MS (continued if necesssary) will meet in S′, in the plane MWS, making SS′ $=$ Ss × Cotang. alt. meteor at s. In either case there will be given, in Problem 1, the angles SWM, WSM, SMW (=180°—SWM—WSM), and SW, to find by plane trigonometry WM, SM. Then the altitude of the meteor above the level of the point w will be represented by Prob. 1.

WM × Tang. elevation of the meteor at w.

In Problem 2, when the points s, w, are at the same level, as in fig. 3, there will be given the angles Mwm, Msm, WSM, and the side SW, to find the angle SWM by the following formula. Prob. 2.

Sine SWM = Tang. alt. meteor at w × Cotang. alt. meteor at s × Sine WSM, which being found, the rest of the calculation may be made as above. When the points s, w, are not on the same level, as in fig. 4, the line SS′ being found as above, and the angle WSS′ equal to WSM or its supplement, SW being also given, the side S′W and the angles WS′S, S′WS, may be found by plane trigonometry. Then as before we shall have

Sine S′WM = Tang. alt. meteor at w × Cotang. alt. meteor at s × Sine WS′M, which gives the bearing of the meteor from W, whence the distance WM, and the height of the meteor as in Problem I.

Remark 2. When either of the azimuths or altitudes is not accurately known, but the limits between which the real value is contained are given; the situation of the meteor may be calculated for each of these limits, and by this means the limiting values of the required elements of the motion of the body may in general be obtained. This method is frequently made use of in this memoir. When either of the azimuths or altitudes is not accurately known.

Remark 3. In order to judge of the accuracy of the results obtained by the preceding problems, it will be useful to repeat the operation, making successively a small change in each of the given quantities. For if any one of the required quantities be not materially affected by these changes, Method of judging of the accuracy of the result.

the calculated value will in general be nearly correct. On the other hand, if a small errour in the observed angles produces a great errour in the result, it will be proper to reject it. Thus, in Problem 1, if the given angles WSM, SWM, are both very small, the least change in either of these angles will in general produce a great change in the situation of the point M, as is well known; and if the two places of observation *s*, *m*, are thus situate with respect to the meteor, the observations made at those places must not be combined together. This is the case with the observations of the Weston meteor made at Rutland and Weston, and for this reason the observations made at those places are not combined together, in the calculations made for determining the place of the meteor.

(To be concluded in our next.)

III.

On the method of JUSSIEU. *By* MRS. AGNES IBBETSON. *In a Letter from the Author.*

To Mr. NICHOLSON.

SIR,

Difficulties of a natural method.

PERFECTLY aware of the extreme difficulty of establishing a natural method in Botany, a task so *often tried without success* by our first botanists, even by Linneus *himself*; the difficulty of following nature, not only in the general appearances the superficial observer may mark, but tracing her in those minute and half *hidden* lines, which require the most *extreme attention*, the most profound knowledge in her ways, the most exact discrimination in her appearances; I cannot but deeply regret, that the long *stride* that excellent botanist Jussieu made, towards obtaining a *natural method*, should be (in any manner) *lost* to the public. But it should be considered, that the life of one man, nay of several, must be far *too short*, to bring to perfection a work of such magnitude; which requires the joint attention of many *physiologists*; who, as the science *advances*, should con-

constantly endeavour, to correct and improve *the plan*; and, applying to its aid every new light thrown on Botany, assimilate the knowledge of all in one grand undertaking.

When therefore we reflect how difficult the task is, it will surely be granted, that even Jussieu had not the *perfect* knowlege requisite to complete such a desideratum, which demands the most exact acquaintance chemically, botanically, and by dissection with every single plant. Drawings embracing most of the appearances are necessary; with the addition of a simple account of those peculiarities, which distinguish the class, the order, the genus, and species; pointing out the various marks, that ally a plant to each different family; mentioning the plants with which it naturally ranks, and *those traits* that form its artificial relations; explaining also its first rise form the *seed*, with all the peculiarities that attend its infant state. Its juices must *next* be *described*; the chemical properties of both *blood* and *sap*; the manner in which they flow; and any variation that may appertain to its *interior structure*; which is perhaps one of the most *curious parts* of a *plant*, and affords us an analogy never before sought. The formation of the flowers must be compared with those of the species not only *externally* but *internally*; on the latter of which *much* of the *habit* of a *plant depends.* The species of root must be shown, with its *interior* formation, and the direction of its strings; as well as the stem, and the variations to which it is subject. The account must mark the first shooting of its leaf and flower *bud*; and whether concealed or not within the *stem*; the effect of the *decayed leaf buds on the stem* in some plants, and the reason why it is not the same in all: the *shape* and *nature* of the full grown flower: when it opens, and closes, and at what times: the history of the *stamen* and *pistil*, and their manner of acting, &c.; *and above all*, each observation must be given in as few short *sentences* as *possible*; which will allow of easily comparing the plants together.

Requisites for this.

When all this is done, I doubt not the *real analogy* will appear: and that we shall then have a *natural method*, not fancied or drawn up by *man*; but *discovered* in *nature*, which will be followed with ease, and, like the *radii of a*

A natural method attainable,

circle branching off, till it embraces in one *grand whole* all vegetable creation.

and should be sought after.

But while I point out the difficulties attending the formation of such a method, still the hope to attain it should not be *laid aside*; and though this great botanist failed in the foundation of *his plan*, he has evinced so much judgment and knowledge in the arrangement of his different *classes*, *orders*, and *genera*, that they may serve as a lesson to all those, who would attempt the task. I shall first therefore detail his method, show the defects of it, and then proceed to point out what appears to me to be the means of rendering it the foundation of future hopes and studies. Jussieu's plan was so simple, and so beautiful, that, while discovering the mistake, I could not but regret that it was not true, and admire the simplicity of the idea. He appeared to have brought together all those marks, both in *physiology* and *botany*, that should discriminate the different classes, all emerging from three great *centres*, appearing to arise as of *themselves* in the *order* and *genera nature would have dictated.* How distressing then to find all this *beautiful structure false*; I really felt deeply the necessity of undeceiving the botanical world, and awakening them from the admiration *the lovely fiction had caused.* Nor was Jussieu to blame; he is amply vindicated by the poorness of his magnifiers, by the knowledge of those times; though perhaps a little *blinded* by the *beauty* of his own *conceptions.* He examined with the powers he *had*; and, seeing all as others had *done*, he *looked no further.* I shall now give a sketch of his method, which will ever distinguish him as one of the first of philosophic botanists. It may be thought, that *I*, an obscure *individual*, am most *presumptuous* in daring to contradict such a genius: but it is not a matter of *argument* or *opinion*, but of *sight*, and having the benefit of better *microscopes* than he had, it is *these* that refute him, not I. He sought truth; and truth is my aim; nor can I believe if living he would not have wished the erroneous part of his method to be effaced.

Jussieu's method.

Jussieu conceived all plants to be separated into three grand divisions, or classes, by their cotyledons or seminal

leaves

leaves: 1st, plants which had no cotyledons: 2d, plants which had one cotyledon: 3d, plants which had two. These were subdivided into orders, which took their distinction chiefly from there being or not being any albumen* in the seed; and this was followed by another separation into genera, by their seed being *farinaceous* or *fleshy*. Hence he proceeds to that strict and excellent separation, which is completely *in nature*, and *truly just*; "whether the stamens are fixed on the receptacle, on the calyx, or *on the pistil*," marking these by the appropriate names of "hypogenes, sur le réceptacle"; "perigenes, sur le calice"; "epigenes, sur le pistil". From this he proceeds to more minute divisions, explanatory of the species, in which he shows his knowledge with a precision and exactness impossible not to admire and feel. But in my last letter I showed, that **His mistake.** these two last are the *only* circumstances of all the method, that have *truth for their basis*; for it is a positive fact, that there are no *certain number of cotyledones*; and though (except the firs) few plants show more than two above ground; yet they are within the seed, and their numbers are rarely regulated but by the length of time the seasons cause the seed to remain in the ground previous to its shooting. He took for cotyledons only the two *last leaves*, appearing from under the *heart*, and as two leaves must always be more or less *in that situation*, he saw not the rest, that were attached to it, or the number which surrounded it. The extreme smallness of the seeds of his *acotyledonous plants* required a nice hand to dissect them, and a *solar microscope* to enable the botanist to discern the figure: and his monocotyledons arose from the *mistake* of taking the *wrong leaf* for a *cotyledon*. As in all other seeds, the impregnating vessel must pass through

* As it may not be recollected or not known to the reader, that in my last letter on seeds I proved, that albumen and alburnum are the same substance; I here repeat it; having taken the soft substance from a graft while joining, and from a seed; and compared it in every chemical way *possible* with so *small a substance*; it proved in every respect the same. Jussieu seems to be of this opinion: for in writing of both, he makes use of but one expression.

the

the corculum; to reach that recess in which most of the cotyledons are formed: in the *nonmammiferous alone*, palms, grapes, and what he calls the *aroides*, the cotyledon part need not pass under the heart but branch off; the cotyledon forming into leaves before the corculum; as in the grasses, or covering it as in the palms, while the primordial stalk alone passes through the heart. As to what he calls his dicotyledons, I do confefs I am astonished at his blindness, for in the walnut, mustard, oak, hazel, they are so large, that the naked eye can almost discern them, and see that there are more than two: and had I not been blinded by my perfect reliance on him, and assurance he must be right, I should not so long have shut my eyes against conviction. I have shown them to many, who were equally with myself convinced, that Jussieu's method was no longer tenable.

Distinction from the albumen in the seed.

I now come to that distinction, which rests on the seeds having or not having albumen. I showed in my last what albumen was in the opinion of the first physiologists, such as Duhamel, Mirbel, and Knight; and explained what I think it to be, *the softer part of the wood*, which when ready prepared for the sap vessels, they would shoot from any neighbouring part into the delicate matter, completing the formation, and it then becomes *perfect wood*, in every sense of the word; and by degrees hardens, and presses closer. There is scarce an experiment possible but I have made with this matter, treating it with *hot* and *cold* water, with spirit of wine, with a sand bath; and I could draw no other inference from its appearance, than the one I have given; which any person may prove the truth of by taking it from a *graft* of *three* or *four months* old before the plant shoots: now as the embryo is almost wholly wood, and the matter is unformed wood, how can some seeds be without albumen? it is impossible: The next part is, that the seeds should be farinaceous or fleshy. Seeds undoubtedly differ much in this respect, instead of being divided into two sorts only; they are separated by nature into four: appearing at first of exactly the same formed matter, though subject to such a variety of chemical changes, as is well worthy the attention of the learned chemist, who discovered

covered the manner of forming sugar in seeds, the truth of which secretion is every day exemplified: though all proceeds from the same apparent matter. As soon as the *embryo shoots*, all farinaceous seeds appear filled with a milky juice, which after a time hardens and becomes a white flower; as wheat, barley, clover, &c.: while in the diadelphian tribe the matter turns to a *deep green*, which hardens also: the 3d may be said to remain as it was at the beginning: while the 4th becomes an absolute oil: this last change appears to rise from the blood of the plant entering the seed for the production of the leaves, but the other variations are certainly most curious, and would make four divisions instead of two: as to the last, of epigenes and perigenes, there cannot be a more distinguishing or more appropriate mark.

I have now given a sketch of Jussieu's plan, and have also shown, that, the last two excepted, dissection proves his distinctions to be wholly false: but though this is the case, yet he has admirably selected his classes, orders, and genera, they will still stand as of themselves from their strong similitude to each other:—and enable us to continue our researches, and draw our conclusion, till farther examination may find a better basis to substitute for the false one: and as no plan of this sort can be perfected, till we have completed our acquaintance by dissection, as well as botanically, with every single plant; yet the nearer we approach to this knowledge, the sooner we shall attain our wishes; for I feel the most *perfect conviction*, that all the regularity found in every part must *arise* from a *natural method* established by the author of all order, and only to be discovered by extreme study and close attention to the *facts* to be found in the vegetable world. There must be a natural method.

I am, Sir,

Your obliged servant,

AGNES IBBETSON.

I have just been cutting a graft of the daphne cneorum on the d. mezerium, where the formation of the albumen was not complete: and as in no plant this process is seen in a plainer Grafts.

plainer manner, I had the delight of showing it to several gentlemen in various conditions: first without any vessels except the bastard pipes, and afterward a little lower with not only bastard pipes, but the silver grain complete, and the sap vessels: but this has been repeated in several grafts—indeed they need but be taken early enough, and they cannot fail to show this effect.

In the observations which I have mentioned as necessary to form a complete account of each individual plant, in the hopes of detecting the analogy necessary to produce the discovery of a natural method, I did not mean to find fault with the botanical description, for certainly Dr. S. has left us nothing to be desired on this head: but anatomically we may be said to be wholly ignorant of plants; this will undoubtedly produce an analogy closer and superior to all except a chemical analysis of its juices*:—to prove this I need give but a few; there is in most plants a place of separation to convey each different juice to that separate part of the flower it is intended to form and nourish. Linneus was of this opinion, and from the first dissections I made, I was convinced of the truth of the suspicion—I have since found it in almost every plant I could obtain, both exotic and native: some have this division under the pericarp, some at the bottom of the flower stalk, and the change is most conspicuous. There is also another curious particular—in all the phaseoli, and a few diadelphian plants, an increase of the joint. I noticed something resembling this in the mimosa sensitiva—but that had but one ball: but in the phaseolus coccinea, the elbow is not more completely jointed than the knots of this plant. The number depends on the smaller stalks shooting from the main stem, for to each there is a ball and socket. This ball turns in almost every direction, and if the large branch is cut, it will show it perfectly. I have seen as far as four balls, and the ease with which the peduncles turn will prove, that it must be something of this kind of formation: but what is most curious in this contrivance is, no sooner does the plant seed, than the ball decays—and if

Different juices conveyed to separate parts of a plant.

* From what we know already of the chemical analysis of vegetables it is obvious, that no assistance in the classification of plants can be derived from this source. C.

it

it does not destroy the stem; this will *contract*, and will long after show the sockets empty.—I have added a figure of the dissection of the stem taken from a large plant of the phaseolus coccinea: see Pl. IV, fig. 1. I could mention many more proofs of curious inward structure, but shall confine myself to the different formation of the corolla, from which most part of what is called the *habit* of a plant *proceeds*. There are various species, *diversified* by their different *formation*, well worth a letter to show it, and as I mean in my next to give a complete sketch of the interior formation of corollas in general, I will not anticipate the subject.

IV.

Remarks on the British Species of Drosera. By ROBERT LYALL, *Surgeon, Member of the Royal Physical Society at Edinburgh, and Corresponding Member of the Literary and Philosophical Society at Manchester. Communicated by the Author.*

IN consequence of the contrary opinions of celebrated botanists, and of the slight variations to which the plants are liable, much ambiguity has been induced respecting our indigenous species of drosera. It is the object of the following remarks, to show by definite characters what should be reckoned distinct British species of this genus of plants.

British species of drosera not well ascertained.

As there can be no dispute, with regard to the d. rotundifolia, I proceed immediately to speak of the d. longifolia.

Linneus questioned whether the d. longifolia was a distinct species, or merely a variety of the d. rotundifolia; and Scopoli states, that the d. rotund. and longif. are only varieties of the same plant, and that he has various specimens, which show the gradation from the one to the other. Haller, on the contrary, will not allow, that these plants are merely varieties: indeed he considers them as distinct species;

Linneus had doubts of the d. longifolia.

Scopoli says it is merely a variety.

Considered as a species by Haller,

and many British botanists.

species*; and his opinion has been supported in Britain by Hudson, Lightfoot, Withering, Smith, and other botanists, if I may judge from the arrangement, which these celebrated men have followed in their different works. As I also concur in sentiment with Haller, it is my duty to mention the reasons which led me to adopt his opinion, and therefore I shall now present a statement of my observations, which was drawn up on the 17th of last July, a day on which I examined some hundred plants.

Reasons for acceding to this opinion.

Number of flowers on the scape of d. rotundifolia,

Of 120 plants of the d. rotundifolia from Paisley Moss,

1 had a scape which bore	2 flowers
4	3 ditto
13	4 ditto
19	5 ditto
17	6 ditto
26	7 ditto
17	8 ditto
16	9 ditto
3	10 ditto
4	11 ditto
120	

Of the same number of plants of d. longifolia, likewise from Paisley Moss,

and of d. longifolia.

5 had a scape which bore	1 flower
20	2 flowers
60	3 ditto
28	4 ditto
6	5 ditto
1	6 ditto
120	

Now, on comparing these tables, the difference in the number of flowers borne by the d. rotundifolia and longifolia must be obvious, for in the one, the scape, for the most part supported from 4 to 9, in the other, only from 2 to 4 flowers.

Differences in the scape,

I shall in the next place notice the scape and leaves.

* See Miller's Dictionary.

From

From the scape of the d. rotund. being more slender, from the greater degree of curvature which the raceme assumes, from the greater number and smaller size of the flowers, than what is observed in the d. longifolia, I can at once tell, although I see no other part of the plant, when the scape is presented, to which of the species it belongs.

Beside the difference of size and form in the two species of drosera mentioned, it may be remarked, that in the *rotundifolia* the leaves in general spread almost horizontally, but in the *longifolia* they grow nearly erect: in the former the petiole or leafstalk is covered all over with long white hairs; in the latter species it is almost naked, a hair only occurring here and there, at considerable distances. and in the leaves.

Being well aware of the influence of soil in metamorphosing plants, I questioned whether the differences, which I have just detailed, between the d. rotund. and long. might not be produced by *soil* alone. On reflection, however, I was obliged to lay aside this notion, for the following reasons. 1st. In White Moss, near Manchester, and in the Paisley Moss, I have often seen the two species growing in contact with each other. 2. I observed the d. *rotund.* in Trafford Moss, and about Kersal Moor (both near Manchester) in very moist soil, though I do not recollect meeting with a single plant of the d. *longifolia.* 3. The plants of the d. longifolia, which bore the fewest flowers, were all dwarfish; showing, that, as the plant diminished in size, so did the flowers decrease in number. 4th. I have never been able, like Scopoli, to trace the gradation, from the one species to the other. These differences not owing to the influence of soil.

However nearly allied the d. *rotund.* and *long.* may seem at first sight, I think, from the reasons above assigned we are fully authorized, to reckon them distinct species: indeed had there been no other difference in the plants, except in the shape of the leaves, we might be justified in founding specific characters on this circumstance alone, according to the Linnean principles.

The following specific characters are sufficient for a *Flora Britannica.* Specific characters.

Drosera rotundifolia. Foliis orbiculatis.

D. longifolia. Foliis oblongis.

I com-

D. anglica first distinguished by Hudson.

I come next to treat of the drosera anglica. Hudson I believe was the first who gave this species its trivial name, and who assigned the specific characters, by which we were to discriminate it from the d. longifolia, of which previous to his time it was only reckoned a variety.

Considered a species by Withering and Smith.

Withering and Smith seem to coincide in opinion with Hudson, for in their works, before noticed, they have followed his arrangement. It is therefore unwillingly, and with due deference to the authority of such superior botanists, that I venture to entertain a different sentiment.

Reasons for dissenting from this.

Among my last years notes, I find the following, "Is the d. anglica a distinct species, or merely a variety of d. longifolia? I have found plants, corresponding to the description of the d. anglica given in Smith's Flor. Britan. &c., and with the figure of Morrison there referred to, which had only six styles and a three valved capsule."

In July last, I carried home a number of plants of drosera, some of which agreed with Morrison's first figure, and others with his second figure, of plate 4th, in size, shape, &c.; and when I examined the largest plants very particularly, I discovered, that one flower had six styles, and another nine, though they belonged to the same scape —that two flowers from another scape had each seven styles, —while another couple from a third scape possessed only six styles each. The styles I found also varied as to number in the smaller plants*.

Number of styles variable,

as well as that of the valves of the capsules.

On the uniformity of the valves of the capsules I could place no dependance; for in the large as well as the smaller plants they varied from three to four in number.

Not being able to perceive any difference in the other parts, as the leaves &c. of the different plants, except in size, I concluded, that what has been denominated the *dros. anglica* is *simply* a variety of drosera longifolia.

Only two British species.

If my conclusion then, be just, it follows, that we have only two British species of drosera, which may easily be discriminated by the characters before given.

Rorella of Ray.

Ray's *rorella longifolia maxima* justly belongs to d. longifolia, and is what was denominated d. anglica. His *rorella*

* It is worth noticing, that in the drosera *rotundif.* the number of styles is, likewise, subject to variation.

rotundifolia

rotundifolia perennis and *longifolia perennis* have, I am inclined to believe, been properly placed by Smith, Miller, &c. under drosera *rotundifolia* and *longifolia*; although I find myself unable to account for the statement given in his work, by Willisell, that some of the plants were annual, and others perennial. Is there no mistake in this account?

P. S. Since I finished these observations, I have procured Dr. Smith's small but excellent *Compendium Floræ Britannicæ;* and find that, after describing the d. anglica, the Dr. adds, "*Anne varietas?*" which plainly shows, that he entertained some suspicions, as to its being a distinct species. I hope my former remarks will afford sufficient answer to the question.

Dr. Smith questions the d. anglica's being a species.

N. B. It is proper to add, that many of my experiments, regarding the irritability of the drosera, related in vol. XXIV, p. 263 &c., of your Journal, were made upon the drosera anglica formerly so called.

V.

Remarks on the Expansion and Closing of the Flowers of the Passiflora cœrulea, or Passion-Flower. By the same.

SOME very curious and interesting phenomena present themselves, from the time that the flowers of the passiflora cœrulea, (passion-flower) begin to open, till the period at which they completely close. As I have nowhere met with a description of these phenomena, I have been induced to communicate a few observations relative to the subject.

Phenomena of the passion flower.

Before entering upon the immediate topic of this paper, I may remark, that the *corolla* of the passiflora cœrulea consists of five petals, and that the *calyx* is formed by five leaves, all of them terminated by crooked spines, which remain in contact, during the infantile state of the flower, or indeed till the flower is ready to burst. On the day that the flower is to open, all the divisions of the *calyx* begin either to part at once or alternately, and afterward expand considerably; when the petals of the *corolla* separate in a

Its state before it opens.

Its mode of expanding.

longitudinal

longitudinal direction about their middle, and soon after disengage themselves at their *apices* and spring suddenly backward; with the *laciniæ* or divisions of the *calyx* accompanying them. Sometimes a number of petals disengage themselves and expand at once, at other times they proceed regularly to burst open one after another.

Changes in the stamens.

What deserves particular notice is the changes which the *stamens* and *pistils* undergo from the opening till the shutting of the flower.

Immediately before the flower opens, the *anthers* present their upper surface, which is covered with yellow *pollen*, toward the *stigmata*, as is shown in plate IV, fig. 2; being retained in that position by the closed petals. But when the petals, which stand opposite to their respective stamens, expand suddenly, which is generally the case, to a considerable degree, then the *anthers*, turning upon the top of their *filaments*, as upon an axis, appear as in fig. 3; with their yellow surface directed toward the heavens; and farther, as the petals gradually or suddenly expand to their full extent, so do the *anthers* become inclined downward, till they appear as in fig. 4, with their yellow surface facing the horizon. Very soon after, the anthers, turning still upon the same axis, acquire a very different appearance; for what was formerly their upper and yellow surface, now becomes directed to the earth. Lastly, the anthers perform half a revolution (by means of a slender membranous string, which attaches them to the filaments described hereafter) and then appears as if suspended by a thread, fixed at their middle; their two ends pointing horizontally, as in fig. 5; whereas, before the complete expansion happened, one end of the anthers (see fig. 4) was directed almost perpendicularly upward and the other downward. The filaments, soon after this circumstance, become a little more curved, and the anthers retain the last mentioned position, till the time at which the flower begins to shut, (which for the most part happens in the course of 22 or 23 hours,) when it claims the attention of the investigating physiologist, to observe the other changes of situation, which the stamens undergo. After remaining nearly a day in the state represented by fig. 5, the *anthers*, by another semirevolution, regain nearly the same position as in fig. 4; the filaments then

then are intirely reflected, agreeably to the view of them in fig. 6. The upper surface of the anthers now is turned toward the columnar receptacle, shown at *a* in the same figure. Very often, however, the *anthers* remain as in fig. 5, when the filaments become recurved and approach the columnar receptacle.

The stamens, after undergoing the changes described, at last are surrounded by the closing nectaries (the pistils suffer an alteration hereafter to be noticed); the nectaries are next covered by the petals, and the petals by the calyx.—These different parts soon become flaccid, the flower withers, dies, and lastly falls off.

Changes in the pistils.

Anterior to, or directly after the opening of the flower, the pistils* are somewhat erect, as is exhibited at fig. 2; but during the day they gradually decline more and more, till their stigmata hang among the anthers, as at fig. 5. In this state they continue for some time, and afterward either previous to, or during the closing of the flower, they retire by degrees, from among the anthers, and resume their pristine situation, as at fig. 2.

To see the pistils in the different aspects noticed, fresh flowers only must be examined; for when the flower fades, the corolla and calyx cease to afford support to the flaccid styles, which consequently fall downward.

Mode of connexion between the anthers and filaments.

I have referred above to the mode of connection between the anthers and filaments, which I shall now proceed to describe. The anther has on its back and middle part two small elevations, with a little cavity between them, from which a slender and very elastic membrane or ligament proceeds to be attached to the top of the filament, likewise between two eminences. This ligament, from its elastic nature and length, admits of motion in all directions. The anthers may be made to perform four or five revolutions before separating from the filament; for the ligament merely becomes twisted.—Hence we easily conceive, how the anthers perform the motions already spoken of.

* I agree with those who refer the genus passiflora to the class and order pentandria trigynia, and therefore mean by pistils what Linneus nominated styles.

Impregnation.

Impregnation. *Impregnation.* Even when the flower first opens, the anthers are burst, and their surface is covered with pollen; but I imagine, that impregnation is not accomplished, till the period when the stigmata hang among the anthers, which, as I have formerly stated, happens during the *anthesis*, or time that the flower is expanded. If this be the case, we have certainly a very striking illustration of the manner, in which the *pistils* accommodate themselves to the *anthers*, in order that they may be fecundated; and consequently of the sexual system.

Have plants volition. Many disquisitions have been instituted, regarding the *volition* of plants; some admitting, while others deny, that they are capable of exerting such a *faculty*. Here I would wish to ask the abettors and opponents of both hypotheses, If the *passion-flower*, while performing the motions described, does not exhibit, as it were, a design to accomplish certain purposes by slow but regularly continued exertions—and of course an effort, at least, analogous to volition? At present I leave the determination of the question to others. Probably at a future period I may recur to this subject.

VI.

On the Influence of Galvanic Electricity in Metallic Arborizations: by C. J. THEODORE DE GROTTHUSS.*

Precipitation of one metal by another. *Sect.* I. THE precipitation of metals in solution, occasioned by the presence of certain other metals, exhibits such a striking analogy to the chemical effects of galvanic electricity, that we cannot avoid the idea of one and the same cause in all these phenomena.

Formation of the leaden tree. Let *z y*, Pl. IV, fig. 7, be a glass tube filled with an aqueous solution of acetate of lead; and *z* a piece of zinc, the lower end of which is immersed in the solution, and which is fastened so as to be retained at the top of the tube. Suppose the ramifications of the arbor saturni to have reached as far as *m*, I shall prove, that it increases by the

Shown to be

* Annales de Chimie, vol. LXIII, p. 5.

immediate

immediate superposition of the metallic particles on the ramifications most remote from the zinc. Suppose *om* to be a particle of oxide of lead in the lower part of the solution, not yet decomposed, and very near the ramifications: the moment the metal, *m*, is deposited on them, its oxigen, *o*, is consequently set free, and the oxidation of the zinc, *z*, at the same time goes on. Now it remains to be known, 1st. whether this atom of oxigen, *o*, be immediately seized by some peculiar impulse, that carries it through the liquid from *m* to *z*; and, 2ndly, how it is transported thither without any appearance of gas: or, 3dly, what becomes of it, if it be not moved out of its place. It would be absurd to suppose, that the oxide of lead, *mo*, came into contact with the zinc, *z*; and that the metal, *m*, after having yielded to it its oxigen, *o*, descended from *z* to *m*; since, in proportion as the arborization takes place in such a narrow tube, the superior part of the liquid no longer contains any lead, but acetate of zinc. Yet, as it would be easy to conceive some kind of flux and reflux, or to invent any other hypothesis, I will endeavour to obviate such suppositions by a decisive experiment, without entering into any tedious discussion.

by additions at the extremities.

Questions.

Untenable supposition.

If a tube, *a b*, fig. 8, be filled with a solution of muriate of tin diluted with water; and a little cylinder of zinc, *z*, be placed at the bottom of the tube; the arborization will go on regularly, often to the height of an inch or more, if the liquid be of a proper density. It is not till after these first rudiments of an arborization have taken place, that the different branches, crushed by their own weight, fall down, and exhibit a confused heap of metallic ramifications. The tree of Diana too grows very regularly from the bottom upwards, but this symmetry is essential, and the precipitation of the metal by superposition is sufficient to establish two propositions; namely, first, what becomes of the oxigen of the metallic particles, that are deposited on the extremities of the tree farthest from the zinc? and, 2dly, what becomes of the disoxigenized principle, at the place where it yields its oxigen to the zinc*? These two questions offer themselves in

Experiment.

Silver tree.

Propositions to be answered.

* We shall see presently, that, almost immediately after the arborization of the dissolved metal has commenced, nothing more is deposited about the zinc, which notwithstanding continues to be oxided; but all

all similar cases, and not alone in that before us. They who supposed in the first experiment, fig. 7, that the disoxigenized metal was conveyed instantaneously from *z* to *m*, or from the top downward; must necessarily admit, that in the second experiment the metal, *m*, after having ceded its oxigen, *o*, to the zinc, *z*, which is at the bottom, must have been carried from *o* toward *m*, that is, from the bottom upward. But these two suppositions are diametrically opposite to each other; and, besides, each is repugnant to the prime law of chemical attraction, according to which the sphere of perceptible affinity between bodies never extends beyond the distance of apparent contact. The absurdity of these hypotheses therefore is evident, and requires no farther discussion.

Erroneous suppositions.

Sect. II. There is another way of explaining metallic arborizations, more plausible indeed, but equally false.

Another hypothesis

It may be supposed, that, in proportion as the metallic particles of the solution are precipitated on the zinc, they are raised up successively by those, that proceed to be deposited on it in their turn, to cede to it their oxigen. In this case the first stratum of metal precipitated must be continually removing farther from that which acts as the precipitant; and all the various changes of form must take place solely in that part of the arborization, which immediately covers the latter metal. But we ſhall see the contrary to this demonstrated by experiment. If, for instance, the extreme ramifications of the tree of Saturn, fig. 7, have arrived as far as *c*, make a mark with ink opposite to this on the outside of the tube, and you will soon see, in a quarter of an hour's attentive observation, fresh ramifications implant themselves on those that before existed; the new accretions will advance beyond the mark, but the extremity *c*, corresponding to it, will remain fixed; and the ramifications most remote from the zinc will be continually undergoing changes of figure, while the part of the tree between the mark and the zinc will not experience the leaſt change. The hypothesis alluded to therefore is untenable, but from the observations made we may infer, that, when a stratum

confuted by experiment.

the fresh particles of revived metal proceed to attach themselves at a distance from the zinc, that is at the last extremities of the arborization.

of

of metal in a solution is once deposited on that, which serves as its precipitant, it remains there immovable during the whole time of the experiment; and that the new metallic particles proceed to plant themselves on those, that already existed, so that the arborization increases only at its extremities.

Supposition that the metal first revived attracts more.

Sect. III. This principle once established, we readily perceive, how the mutual attraction of the two metals, added to the predominant affinity of the one for the oxigen and acid of the other, fully explains, in most cases, the revival of the first particles, but not of the subsequent ones; and that the employment of all these forces is insufficient to solve the two principal problems. Still they have another defence, who suppose the attraction of one and the same metal sufficiently intense, to act the principal part in the precipitation of metals by one another. They might say, that, in proportion as the metal in solution is brought to deposit itself on that already precipitated, the oxigen of the former remains dissolved in the liquid, and the zinc slowly and gradually lays hold of it.

Experiment to confute this.

Unfortunately the following experiment destroys these fine efforts of the imagination. Fill the tube *ab*, fig. 9, with an aqueous solution of muriate of tin, and immerse in it the zinc cylinder *z*; in the space of half an hour the cylinder will be covered with a fine tin arborization. Next separate these ramifications from the zinc, without destroying them: which may easily be done by dexterously turning the little cylinder: and let them sink gently as far as *c*, keeping them from falling quite to the bottom by giving the tube a proper inclination. It is essential, that the zinc remain immersed in the liquid, as in fig. 9. In this state the apparatus may continue hours, days, and even weeks, without the arborization *c d* receiving the least perceptible addition, though it is surrounded on all sides by oxide of tin in solution. If you have taken the precaution to pour water gently into the upper part of the tube, so that it does not mix with the muriate of tin occupying the lower part from *b* to *c*, the zinc may remain a long while in the water, without being covered with fresh ramifications of tin. Now if the attraction of one metal were sufficient to assimi-

 late

late that which is in solution; and the oxigen, of which it is divested, remained in the liquid, to be gradually conveyed to the precipitant metal; the particles of tin ought to continue to be deposited on *c d*, as when this part was in contact with the zinc. But as no effect takes place, after this contact is broken off, this hypothesis is as unfounded as the preceding.

A contact of metals essential,

This simple experiment clearly demonstrates, that the metal in solution cannot continue to deposit itself on the extremities of the arborization *c d*, unless this communicates at some point with the metal *z*, which acts as the precipitant. But it is well known, that such a contact of two metals is sufficient, to give rise to a current of galvanic electricity; and that the effects of this electricity are identical with those observed in the precipitation of one metal by another: here then we have one proof, that this phenomenon depends on a similar cause. The following experiments fully confirm this conclusion.

which excites a current of electricity.

Farther experiments to show this.

Sect. IV. I filled half a tube six inches high, and a little more than a quarter of an inch in diameter, with a concentrated solution of nitrate of copper. Into the other half I poured very gently a solution, not very dense, of pure and perfectly neutral crystallized nitrate of silver. With the latter I had mixed a little alcohol, to diminish its specific gravity; and thus I was enabled to place the two liquids in contact without mingling them. That in the lower part of the tube was of a fine blue colour; that above was clear and colourless. In the middle of the latter, that is about an inch and half down the tube, I placed a small cylinder of copper, which soon began to precipitate metallic silver in the form of very delicate and fine garlands. These chains of silver flowers gradually approached the blue solution of copper, and, in twenty-four hours after they had reached it, I had the agreeable surprise to see their extremities covered with particles of copper, the colour of which was perfectly distinct, so that they could not be confounded with those of silver. The tree of Diana, which in this experiment appeared to have been transplanted into the garden of the Hesperides, acquired in four days an increase of a quarter of an inch, the colour and

Arborizations of silver

tipped with copper.

and ramifications of which were unquestionably those of copper. This experiment I repeated, employing a cylinder of zinc instead of copper, and then the precipitation of the copper at the extremities of the branches of silver was much more speedy.

Hence it evidently follows, 1st, that the arborization of metals, precipitated in the perfect state from their solvents, is formed by superposition of their particles on one another: 2dly, that, since neither copper nor silver has of itself the power of reviving the metal contained in nitrate of copper, these two metals were rendered capable of producing this effect only in virtue of a current of galvanic electricity, resulting from their mutual contact. Arborization a superposition of particles effected by galvanism.

The following is a farther proof of the truth of this induction. Establish a communication between a plate of copper and a plate of silver, both well polished, by one of their extremities, and immerse the other two extremities in a solution of my salt of copper, in such a manner that there shall be the space of an inch between them. After leaving them thus four and twenty hours, the silver will be distinctly covered with a coat of copper, and the other metal will be oxided*. To render this effect still more evident, I place a few particles of nitrate or sulphate of copper, slightly moistened, between a silver coin and a plate of copper, both previously well cleaned with acid. I then tie them, or compress them together, and leave them for a few hours. On separating them, the silver will be found covered with a coat of metallic copper, very distinguishable by its colour. It is a curious fact, that a salt with an earthy or alkaline base may be employed instead of a salt of copper; but in this case it is obvious, that the acid of the salt first takes up a little of the copper, and then depo- Farther proof. Silver coated with copper. An earthy or alkaline salt between the two metals will answer.

* Dr Wollaston, who first observed the chemical action of two metallic plates immersed in solutions of different metals, supposed, that one of them must possess the property of precipitating the metal in solution, in order that the other might coat itself with it also by means of a conducting power. The experiment I have related however proves, that this is not a necessary condition; and we may obtain in a few minutes the revival of zinc, by placing the sulphate of this metal moistened with water between a plate of zinc and a plate of copper. It is not necessary that the metal in solution should be precipitable by the other.

sits

sits it on the silver. It follows from these facts, that the galvanic electricity excited by the contact of two metals is sufficient, to effect the precipitation of a metal held in solution, which would not have been precipitated by either of the metals taken separately.

Theory of the galvanic decomposition of water.

Sect. V. It now remains to solve the two problems in Sect. I. Perhaps the reader will have already remarked, that they are nearly the same as present themselves in the galvanic decomposition of water, and that they may be all solved on the same principles. On this account I think it necessary to recapitulate in a few words the theory I gave of the latter phenomenon in a short paper in the Ann. de Chim. for April, 1806. I first proved, that water, traversed by a current of galvanism, becomes oxigenized at the positive pole, and hidroguretted at the negative pole, Though the contrary has been presumed; and though it has been said, that it becomes neither oxigenized nor hidroguretted; my recent experiments appear to me evidently to confirm the principle I have announced. I will relate only two.

Proofs that the water is oxigenized at the positive, and hidroguretted at the negative pole.

a. Hidroguretted sulphuret of barytes, exposed to the action of the pile, affords gradually round the positive pole an abundant quantity of a white precipitate, which has all the characters of sulphate of barytes; while the liquid round the negative pole becomes evidently clearer, and ultimately colourless.

b. If the galvanic current traverse an alkaline solution of indigo, made by means of some disoxigenizing substance, immediately there is formed at the zinc pole a precipitate of a fine blue colour, which is capable of being completely redissolved, if the respective places of the two poles be changed*.

Water, when galvanized, becomes a secondary pile.

Sect. VI. Water becomes a real secondary pile while it experiences the galvanic action. On this occasion we may recollect the remark of Volta, that a piece of wet paper, each extremity of which touches one of the poles of the pile, retains for some minutes the charge received, even

* In these trials I have never immersed the poles, by which term I mean the conducting wires, more than four or five lines below the surface of the liquid.

when

when it is no longer in contact with the apparatus. Pure water in a curved tube, which has been placed in the galvanic circuit, likewise retains for a short time the power of contracting the muscles of a prepared frog. I conceive therefore, that its galvanic decomposition is effected in the following way. Let us suppose a thread of water formed of three integrant particles, fig. 10, each composed of two atoms of oxigen, represented by $\overset{\circ}{\circ}$, and one of hidrogen, represented by *h*: and let it be observed, that what is said of this thread may be afterward applied to any quantity of this fluid.

Mode in which its decomposition is effected.

The moment a galvanic current is established, the electric polarity manifests itself between the elementary particles of the water, so that they appear to be the complement of the pile in action. Now since the oxigen is attracted by the + or zinc pole, and hidrogen by the — or copper pole, we are naturally led to infer, that each of the two principles requires, at the instant of its production, an electrogalvanic state opposite to the poles which attracts it; and as this reasoning may be extended to all the particles of water in the direction of the galvanic current, it follows, that, if we take them two by two, the homogeneal principles will repel each other, and the heterogeneous principles attract each other, alternately*. Thus as soon as the oxigen, $\overset{\circ}{\circ}$, passes to the state of elastic fluid in consequence of the attraction of the pole endued with vitreous electricity, its hidrogen *h*, by virtue of this electricity, repels its similar *h'*, and enters into combination with the oxigen, $\overset{\circ}{\circ}'$, of the adjacent particle; at the same time *h'*, being repelled, transmits its movement to the homogeneous atom, *h''*, and itself unites with the oxigen, $\overset{\circ}{\circ}''$, of the third particle, the hidrogen of which, *h''*, must finally assume the state of gas, since it is in immediate contact with the pole charged with resinous electricity. In the whole of this process, it appears, the particles of water at the extremities of the conducting thread are alone decomposed, while the intermediate ones reciprocally change their component principles in succession, but without altering their nature. This theory agrees

* The idea of this polarity, analogous to that which exists in the pile, I conceive distinguishes the explanation I here give from any other.

completely

completely with the facts, which prove beyond question, that the water becomes oxigenized at the positive pole, and hidroguretted at the negative: for, since the oxigen of the whole quantity of water traversed by the galvanic current tends incessantly, to proceed toward the pole endued with vitreous electricity; and the hidrogen is similarly affected with regard to the wire charged with resinous electricity; it evidently follows, that the water must become oxigenized at the zinc pole, and hidroguretted at the copper pole.

This consistent with the laws of chemical affinity.

Sect. VII. In this theory of a chemical phenomenon every thing is conformable to that law of affinity, by which the radius of its sphere of action never extends beyond the distance of apparent contact. The effects of this attractive power however may become sensible at a distance, when it is transmitted by *dissimilar* particles; as the effects of repulsion may be by the transmission of the corpuscular action by *similar* particles. Elastic bodies, that possess a certain mobility analogous to that of the particles of a liquid, afford a striking example of a transmitted motion, which does not become sensible, till it has reached a certain distance from the point of its origin. If a ball of ivory, suspended by a thread, be let fall against a row of similar balls, suspended in the same manner, and in contact with each other, the impulse given to the first will be immediately communicated to the second, from which it will be transmitted to the third, and so on to the last ball; which, receiving the motion, and having nothing to communicate it to, will fly off with a force equal to the primary impulse. This well known law of mechanics may be happily applied in all chemical phenomena, where the object is to explain a force, the effect of which is become sensible at a distance.

Example of transmitted motion in mechanics.

Theory of the precipitation of one metal by another.

Sect. VIII. The theory of the galvanic decomposition of water, which I have just explained, is that of the precipitation of metals by one another. With regard to the revival of the first particles of the metal held in solution, I shall offer but one remark in the next section, since Berthollet and Vauquelin have already treated the subject in such a superior manner*: but from the facts and reasonings

* Essay on Chemical Statics, vol. II, p. 421; and Annales de Chimie, vol. XXVIII.

given

given above, it is impossible to doubt, that the subsequent metallic arborization is owing to an electrogalvanic polarity, which is established between the elementary particles of the liquid comprised between the precipitant metal and the extremities of that which is precipitated. The oxidation and revival manifest themselves in this process at a distance more or less considerable, according to the progress of the arborization; while all the intermediate component particles of the liquid, in consequence of this polarity, undergo a mutual exchange of their elementary principles. If it were required to render visible the positive electricity of the precipitant metal, the extremity z' of which, fig. 7, might be made to communicate with a sensible electrometer; it should be remembered, that this is impossible, because the metal is immersed in the liquid, and the revival of that in the solution is effected in consequence of the galvanic circle being then closed, so that it is in the state of a pile, the two poles of which communicate by means of a conducting substance. The fine experiments of Volta show, that the contact of two metals is sufficient, to excite a current of electricity; and all that illustrious philosopher demonstrated with plates is perfectly applicable in this case to the elementary particles of water. It is sufficient therefore, for the complete solution of the two propositions in my first section, to conceive the idea of an electric polarity established between these particles as often as they are attracted by polar electricity*.

Why the electricity does not affect the electrometer.

Sect. IX. Alcohol containing any metallic salt, as acetate of lead for instance, powerfully opposes the precipitation of the metal that is held in solution, whether a piece of zinc be left in it, or it be galvanized. This appears to me to prove, that the elelectric polarity manifests itself between the component particles of the *water*, and not of the *oxide*, as I was myself inclined to think some time ago. At present I am of opinion, that the precipitant metal acquires the positive state by contact with the oxigen of the water, and that it attracts this oxigen from being in the opposite state:

Alcoholic solutions of metals not precipitated like aqueous.

* This expression appears to me well adapted to designate galvanic electricity.

state: the hidrogen being on the contrary repelled, because it also acquires the vitreous electrity, attracts for the same reason the oxigen of the metallic oxide held in solution by the acid and water; whence it follows, that the metal, being divested of its oxigen, must reappear in its perfect lustre.

Quantity of caloric in an oxide probably proportionate to the oxidability of the metal.

A circumstance not to be forgotten in this explanation, but to which I cannot at present assign its just value, is the variable quantity of caloric retained by metallic oxides. It appears probable to me, that this quantity is in the inverse ratio of the oxidability of the metal. The following is the manner in which Berthollet expresses himself on this head in his excellent Essay on Chemical Statics, vol. II, p. 382.

Berthollet's remark

"Oxigen retains a larger or smaller quantity of caloric in its combination with metals, as well as in those it forms with other substances. On this depend some of the properties, that distinguish oxides in their relation to combustible substances. Those of gold, silver, and mercury, retain a great deal; whence their compounds with ammonia detonate either by a slight rise of temperature, or even by compression. The oxide of copper, which also is rendered capable of decomposing ammonia by an increase of temperature, produces however no detonation; which shows, that the oxigen in it is much more deprived of caloric. The oxide of silver detonates more forcibly than that of gold, or of mercury; so that the oxigen appears to retain more caloric in the former, than in the latter."

questioned by Brugnatelli.

Brugnatelli, whose sagacity is generally acknowledged, objects to this theory, that we observe no evolution of caloric when silver is revived by means of copper; and thence he concludes in favour of his own theory of thermoxigen. This observation appeared to me so decisive, that I had completely adopted the opinion of Brugnatelli in this respect.

Evolution of caloric not always perceptible:

However, reflecting one day on this interesting subject, I recollected, that a thermometer, placed under a receiver in which a vacuum is made, is but little affected by the cold produced by the dilatation of the air, and it occurred to me, that the caloric, which is extricated in the precipitation of one metal by another, may be so quickly absorbed by the surrounding bodies, as not to become sensible. Hence I conceived, that its dispersion might be prevented by diminishing

ing the bulk of these bodies. To prove this, I made a saturated solution of pure nitrate of silver, crystallized and perfectly neutral, in distilled water; and with this solution I filled a very thin tube, 108 mill. [4·25 inches] long, and 7 mill. [2·75 lines] in diameter. After satisfying myself, that the solution occasioned no change in blue paper, I let fall to the bottom of the tube a small cylinder of well cleaned copper, 14 mill. [5.5 lines] long, and 7 mill. [2·75 lines] in diameter. Immediately the cylinder was covered with a forest of Diana's trees, and on touching the corresponding part of the tube the heat was very sensible. Having substituted a zinc instead of a copper cylinder in another experiment, the effect was much more considerable; and what appeared to me very singular was, that, in proportion as the bulk of the silver arborization increased, I was obliged to move my fingers towards its extremities, to be able to feel the heat. These experiments appear to me to prove, that the quantity of caloric evolved in the precipitation of metals by one another is equal to the difference that exists between the caloric that was contained in the oxide of the metal revived, and that which is absorbed by the oxide of the precipitant entering into solution.

but it is in the revival of silver by means of copper in a thin slender tube.

Sect. X. It only remains for me to remove two difficulties, that may be objected to the preceding theory, which embraces almost all the chemical effects of galvanic electricity. I proceed to the first.

Two objections to the theory.

a. It has been observed, that, when the two poles are immersed in separate glasses filled with water, no gas is evolved, because the communication necessary for the discharge of the pile is interrupted. To remove this obstacle, we have only to immerse two fingers of the same hand in the two glasses, and immediately the gasses will appear. This observation has been the stumbling block of all theories, as it was highly puzzling to explain, 1st, what became of the hidrogen in the glass in which the zinc pole evolved pure oxigen; and 2dly, what on the other hand became of the oxigen in the glass in which the copper pole equally evolved pure hidrogen. A little sagacity, and a clear idea of the establishment of the electrogalvanic polarity between the elementary particles of water, will solve this first difficulty,

The 1st

answered.

so

so far as to find in it a new confirmation of the truth of the theory here given. Let us first take the simplest case, and suppose, that we have established the communication between the two glasses of water by means of a piece of wet paper or linen. It is evident, that in this case the water contained in these two substances alone produces the communication; since the linen and paper, when thoroughly dry, comport themselves as nonconductors, or at least as badly conducting substances. Thus there is nothing here to perplex us, except a delusive appearance; and every thing ought to go on, as if the poles were immersed in a single vessel of water. Now let us substitute our fingers instead of wet paper, and consider, that all the muscular parts of an animal are impregnated with an abundant quantity of aqueous fluids, the elementary principles of which are equally capable of acquiring the galvanic polarity, and consequently experiencing the same successive exchange as those of the water contained in the paper. These considerations are sufficient to show, that, in the experiment which at first sight appeared incompatible with our theory, every thing is in reality conformable to it. The property of almost all imperfect conductors impregnated with moisture, to become secondary piles when they are placed in the circuit of the secondary pile of Volta, proves the real existence of the electrogalvanic polarity between the particles of water: but what evinces, that the communication between the water in the two glasses is established solely by the aqueous humours with which our organs are impregnated, is, if we substitute for these organs a dry substance, though a good conductor, as a wire of gold or platina, the developement of hidrogen and oxigen both will immediately take place in each of the two vessels.

2d objection

b. The second and much inferior difficulty consists in accounting for the difference perceived between the decomposition of water produced by an electric machine, and that effected by the pile. In the first the two principles of water, according to Dr. Wollaston, are always evolved mixed together; while the pile separates them in a pure state, and at a distance from each other. The reason of this no doubt is, in the electromotive apparatus the two electric fluids are immediately in contact, whence follows an uninterrupted

answered.

rupted electric polarity, capable of generating a similar polarity between the elements of water; while in all electrical machines, and even in the Leyden phial, the two electricities are always separated from one another by an insulating body, as glass, air, resin, &c. Insulated electricity always acts at a distance, and glides with great swiftness over the surface of conducting bodies: the galvanic fluid on the contrary, which might be more appropriately termed polar electricity, acts on the same substances only when in contact, and by transforming them into secondary piles. This analogy of action at a distance and in contact is observed likewise between the attraction of the heavenly bodies and chemical affinity; which is the more remarkable, as there is no inconsistency in considering the light, that proceeds from those stars that form a part of the universe, as an effect of electricity.

Gravitation compared with chemical affinity.

It appears to me probable, that, in the galvanic decomposition of water, each of the two component principles of this liquid combines intimately with the electric fluid of that pole, by which it is attracted; and that they are both converted into the elastic state in consequence of this combination. If this were demonstrated, the light produced in the combustion of hidrogen gas would also be explained by the prompt reunion of the two electric fluids set free.

When water is decomposed its principles combine with the electric fluid.

Hence the light on burning hidrogen.

VII.

Observations on the Effects of Magnesia, in preventing an increased Formation of Uric Acid; with some Remarks on the Composition of the Urine. Communicated by Mr. WILLIAM T. BRANDE, F.R.S., *to the Society for the Improvement of Animal Chemistry, and by them to the Royal Society.**

MR. Home's inquiries into the functions of the stomach, and his discovery of liquids passing from the car-

Calculous complaints

* Abridged from the Phil. Trans. for 1810, p. 136.

avoided by preventing the formation of uric acid.

diac portion into the circulation of the blood*, led him to consider, that the generality of calculous complaints might possibly be prevented by introducing into the stomach such substances, as are capable of preventing the formation of uric acid; and that this mode of treatment would have many advantages over the usual method, which consists in attempting to dissolve the uric acid after it is formed.

Magnesia adapted to this purpose,

He consulted Mr. Hatchett on the substance most likely to produce this effect, and asked if magnesia, from its insolubility in water, was not well adapted for the purpose, as it would remain in the stomach, until it should combine with any acid, or be carried along with the food towards the pylorus.

as shown by experience,

Mr. Hatchet knew of nothing more likely to produce the desired effect; and on putting this theory to the test of experiment, it was found by a very careful examination of the urine, that in several instances, where there was an increased formation of uric acid, magnesia diminished it in a much greater degree, than had been effected by the use, and that a very liberal one, of the alkalis, in the same patient.

on several occasions.

This circumstance led Mr. Home to wish for a more complete investigation of the subject, and he requested me to assist him in the prosecution of it. Since that time many opportunities have occurred of carrying on the inquiry during an attendance on patients, labouring under calculous complaints.

It is proposed to lay the results of our joint labours before this Society, with a view to establish a fact of so much importance in the treatment of those diseases.

The four following cases include the principal varieties of the disorder, which have been met with, and are therefore selected from among many others, to prevent unnecessary repetitions. In each of them the urine was occasionally carefully analysed.

1st case.

Case I. A gentleman, sixty years of age, who had been in the habit of indulging in the free use of acid liquors, had repeatedly passed small calculi composed entirely of uric acid; his urine, immediately after being voided, de-

* Philos. Trans. 1808. See Journal, vol. XX, p. 374, and XXI, 103.

posited

posited at all times a considerable quantity of that substance, in the form of a red powder, and occasionally in larger crystals.

Nine drachms of subcarbonate of soda, dissolved in water highly impregnated with carbonic acid, and taken in the course of the day at three doses, appeared to have no effect whatever on the formation of uric acid; the red sand was deposited as usual, and the small calculi continued to form.

On account of the inefficacy of this medicine, he was advised to try the vegetable alkali, and three drachms of subcarbonate of potash dissolved in water, slightly impregnated with carbonic acid, were taken at similar intervals.

The deposit of uric acid in the urine was now somewhat diminished; but during this free use of alkalis, which with little interruption was persevered in for more than a year, the small calculi still continued to be voided.

The magnesia was then tried, fifteen grains being given three times a day in an ounce and half of infusion of gentian. In a week the uric acid was found to have diminshed in quantity, and after the first three weeks it was met with but occasionally. The use of the magnesia was persevered in for three months, during which time no calculi were voided, and there was no material deposit in the urine.

This patient was extremely subject to heartburn, and he likewise complained of a sense of weight and uneasiness about the region of the stomach, both of which symptoms have disappeared.

Case II. A Gentleman, about 40, during four years had occasionally voided considerable quantities of uric acid, in the form of red sand, and had once passed a small calculus. He took carbonate of soda dissolved in water highly impregnated with carbonic acid for some time; to which *pure* potash was afterward added: but with little effect. Recourse was then had to magnesia, twenty grains of which, taken night and morning, for six weeks, removed all the superabundant uric acid, though he put himself under no unusual restraint in his mode of living. Case 2.

Case III. A gentleman, 43 years of age, after taking violent exercise on horseback, was seized with pain in the right Case 3.

right kidney and ureter. In the course of the night he passed a small uric calculus. Having had recourse to soda water with little benefit, he took twenty grains of magnesia every night. His urine not being quite free from red sand in the course of three weeks, he took an equal dose of magnesia every morning, and in a month after this his urine was perfectly healthy. In about three months the uric acid again made its appearance in the form of red crystalline sand, and was again removed by the use of the magnesia night and morning for three weeks.

Case 4. *Case* IV. A gentleman, aged 56, after recovering from a severe fit of the gout, voided constantly a large quantity of mucus in his urine, a symptom which he had never before noticed. There was also, occasionally, abundance of red sand, consisting principally of uric acid, but he had never voided a calculus.

His stomach was uncommonly weak, he was often affected with heartburn, and an almost constant pain in the neighbourhood of the right kidney. He had been in the habit of taking tincture of bark, and other spirituous medicines, from a belief, that the pain in his right side arose from gout in the stomach.

He had already attempted to use the alkalis, which had produced such unpleasant sensations in the stomach, that he could not be prevailed upon to try them again in any form.

Under these circumstances, he readily acceded to a new plan of treatment. He was directed to omit the use of spirituous medicines, and take twenty grains of magnesia, three times a day, in water; but this operating too powerfully upon the bowels, the same quantity of magnesia was taken twice a day only, with an addition of five drops of laudanum to each dose.

This plan was pursued without intermission for three weeks, and he received considerable benefit, as far as concerned the state of the stomach, and pain in the region of the kidney. The urine, which was examined once a week, was also, on the whole improved, but it occasionally deposited a very copious sediment, consisting of uric acid, with a variable proportion of mucous secretion.

After

After a farther continuance of the use of the magnesia for three weeks, the urine was often much loaded with uric acid and mucus; but these appearances, which before the use of the magnesia were constant, are now only occasional, so that the disposition to form a redundant quantity of uric acid is much diminished: it is also deserving of remark, that there has not been the slightest symptom of gout from the time of the last attack, which is more than a year back, a longer interval of ease, than this patient has experienced for the last six years.

He has now omitted the regular use of the magnesia, but on perceiving any unpleasant sensation in the stomach, he returns to it for a week or ten days, and then again leaves it off.

From the preceding cases it appears, that the effects of magnesia taken into the stomach are in many respects different from those produced by the alkalis in those patients in whom there is a disposition to form a superabundant quantity of uric acid. Remarks.

With a view to ascertain their comparative effects on healthy urine, when taken under the same circumstances, the following experiments were made.

Experiment 1. *On Soda.*

Two drachms of subcarbonate of soda were taken on an empty stomach at nine o'clock in the morning, dissolved in three ounces of water, and immediately afterward a large cup of warm tea. Action of subcarbonate of soda on the urine.

In six minutes, about one ounce of urine was voided; in twenty minutes six ounces more; and after two hours, a similar quantity.

The first portion became very turbid, within ten minutes after it had been voided, and deposited a copious sediment of the phosphates, in consequence of the action of the alkali upon the urine. It slightly restored the blue colour to litmus paper reddened with vinegar: the alkali therefore was not merely in sufficient quantity to saturate the uncombined acid in the urine, and consequently to throw down the phosphates, but it was in excess, and the urine was voided alkaline.

 The

The urine voided after twenty minutes also deposited a cloud of the phosphates; but the transparency of that voided two hours after the alkali had been taken was not disturbed.

Here, therefore, the effect of the alkali upon the urine was at its maximum, probably in less than a quarter of an hour after it had been taken into the stomach, and in less than two hours, the whole of the alkali had passed off.

Experiment 2. On Soda, with excess of Carbonic Acid.

Action of the supercarbonate of soda on the urine.

The same quantity of soda, dissolved in eight ounces of water, very highly impregnated with carbonic acid, was taken under the same circumstances as in the former experiment, and the urine was voided at nearly similar intervals.

The separation of the phosphates was less distinct, and less rapid. In two hours after the urine had been voided, there was a small deposit, composed principally of phosphate of lime; there was also a distinct pellicle on the surface, consisting of the triple phosphate of ammonia and magnesia. This appearance, produced by the escape of the carbonic acid, which had before retained the ammoniaco-magnesian phosphate in solution, and which now occasions its deposition on the surface, is by no means uncommon, even in the urine of healthy persons; in the present instance, it appears to prove, that carbonic acid passes off from the stomach by the kidnies, for after taking the alkalis in water very highly impregnated with it, the pellicle is uniformly produced, and is also much more abundant and distinct, than under any other circumstances.

Potash similar in its action.

In similar experiments with potash, the results were in all cases as similar as could be expected in researches of this nature.

Experiment 3. On Magnesia.

Action of magnesia on the urine.

Magnesia was taken under circumstances similar to those of the soda in the former experiment: in the quantity of half a drachm, it produced no sensible effect upon the urine during the whole day. When taken in the dose of a drachm

drachm at nine o'clock in the morning, the urine voided at twelve o'clock became slightly turbid: at three o'clock the effect of the magnesia was at its maximum, and a distinct separation of the phosphates took place, partly in the form of a film, which when examined was found to be the triple phosphate of ammonia and magnesia, and partly in the state of a white powder, consisting almost entirely of the triple phosphate and phosphate of lime.

The effect of large doses of magnesia, in producing a white sediment in the urine, is very commonly known, and has been erroneously attributed to the magnesia passing off by the kidnies.

These experiments show, that magnesia, even in very large doses, neither produces so rapid an effect upon the urine, nor so copious a separation of the phosphates, as the alkalis; on this its value as a remedy in calculous disorders seems materially to depend.

Experiment 4. *On Lime.*

Two ounces of lime water, taken in the morning upon an empty stomach, with a cup of milk and water, produced no effect whatever. Action of lime on the urine.

A pint of lime water, taken at four intervals of an hour each, produced a slight deposition of the phosphates at the end of the fifth hour. The urine voided at the third hour was not at all affected; at the fifth hour, the effect appeared at its height, but was not nearly so distinct as from small doses of soda, notwithstanding the insoluble compounds, which lime might be expected to form with the acids in the urine.

The unpleasant taste of lime water, the quantity in which it requires to be taken, on account of the small proportion of the earth which is held in solution, and the uncertainty of its effect, are circumstances which render it of little use, excepting in some very rare cases, where it has been found to agree particularly well with the stomach.

The effect of carbonate of lime upon the urine was much less distinct than that of lime water: at times it produced no effect, but when taken in very large doses, a slight deposition of the phosphates was produced. Action of chalk on the urine.

 These

The results were uniform in three individuals.

These experiments were repeated upon three different individuals, and there was always a uniformity in the results.

When the medicines were taken some hours after food being received into the stomach, their effects upon the urine were retarded, but not prevented.

Other substances tried.

The effects of many other substances upon the urine were examined into during this investigation, but they varied so much according to circumstances, that no satisfactory results were produced.

As it is found in the foregoing experiments, that the effects of soda on the urine are modified by the presence of carbonic acid, the following experiment was made, to ascertain, whether any sensible effects are produced by that acid on healthy urine.

Action of carbonic acid on the urine.

Twelve ounces of water very highly impregnated with carbonic acid were taken upon an empty stomach at nine o'clock in the morning. At ten o'clock about eight ounces of urine were voided, which had a natural appearance; but, when compared with urine voided under common circumstances, was found to contain a superabundant quantity of carbonic acid: this gas was copiously given off when the urine was gently heated, or when it was exposed under the exhausted receiver of an air pump.

Sometimes useful.

In a patient who had a calculus of large dimensions extracted from the bladder, composed entirely of the phosphates, and whose stomach did not admit of the use of stronger acids, carbonic acid was given in water; it was found peculiarly grateful to the stomach, and upon examining the urine during its use, the phosphates were only voided in solution, but when at any time it was left off, they were voided in the form of white sand.

VIII.

Observations and Experiments on the Nature of Oximuriatic Acid, and its relation to Muriatic Acid, by J. MURRAY, *Lecturer on Chemistry, Edinburgh.*

To Mr. NICHOLSON.

SIR.

Chemical theory liable to change.

THE theories of chemistry, resting on evidence not strictly demonstrative, are liable to frequent modification from

from the progress of discovery. Opinions however more unexpected have seldom been announced to chemists, than those lately advanced by Mr. Davy with regard to the constitution of muriatic and oximuriatic acids—that the latter is not a compound of muriatic acid and oxigen, but a simple substance, and that the former is a compound of this substance with hidrogen. The more general principle connected with these opinions—that oximuriatic acid is, like oxigen, an acidifying element, which forms an extensive series of analogous compounds with inflammables and metals, leads still more directly to the subversion of the established chemical systems, and to an entire revolution in some of the most important doctrines of the science.

Opinions of Mr. Davy on the muriatic & oximuriatic acids

important as introducing a new acidifying principle,

Opinions so novel and important claim attention; and this is not less due to them from the authority on which they are advanced. They derive too an additional interest from their connection with a part of chemical theory at present involved in much obscurity—the relation of water to the constitution of some of the gasses, and more particularly muriatic acid gas. Having been favoured some time ago by Mr. Davy with the Memoir, in which these speculations are announced, I have had it in my power to submit to examination the evidence on which they are supported, and to prosecute an experimental investigation to which this led. Some account of the results of these researches may not be unacceptable to your chemical readers; and to Mr. Davy, I am confident, I need offer no apology, for bringing under review a question of such interest, and at the same time of such a nature, that when surveyed under different aspects it may suggest very different conclusions.

and as connected with the relation of water to the constitution of some of the gasses.

The common phenomena, with regard to the production of oximuriatic acid, and its apparent analysis, admit of explanation equally on the established theory, and on that advanced by Mr. Davy. It is necessary therefore to take notice only of those, which are more peculiar; and, supposing your readers acquainted with Mr. Davy's memoir, I shall confine these observations to the facts, which he has stated as favourable to his opinion.

The common phenomena explicable on either theory.

Facts considered as most favourable by Mr. Davy.

When oximuriatic acid gas and hidrogen gas are mixed in nearly equal volumes, they act on each other, and are entirely

Composition of muriatic acid gas from the

oximuriatic acid and hidrogen, not decisive:

tirely converted into muriatic acid gas. This experiment Mr. Davy regards as synthetic, the oximuriatic acid and hidrogen forming he supposes the muriatic acid by their combination. There is obviously however another mode of accounting for the result. Regarding oximuriatic acid as a compound of muriatic acid and oxigen, the oxigen may be supposed to combine with the hidrogen and form water, while the muriatic acid remains in the elastic state. Mr. Davy has remarked indeed, that, when the gasses have been rendered as dry as possible, there is no sensible deposition of water; but no difficulty attends the supposition, that this may arise from the portion of water which is formed being retained in combination with the acid gas.

or the decomposition of sulphuretted hidrogen by oximuriatic acid gas:

Oximuriatic acid gas decomposes sulphuretted hidrogen, sulphur is deposited, muriatic acid gas is produced, and, when the gasses have been previously dried, the condensation Mr. Davy found is not equal to $\frac{1}{10}$. This decomposition, in conformity to his hypothesis, must be supposed owing to the hidrogen combining with the oximuriatic acid, and forming muriatic acid. According to the common theory the results must be ascribed to the oxigen of the oximuriatic acid combining with the hidrogen and forming water, which the muriatic acid holds combined with it; and the experiment presents nothing inconsistent with this explanation.

or that of ammonia by the same gas:

Ammonia, it is well known, is decomposed by oximuriatic acid. Mr. Davy has stated, that, in making this experiment, he has found, contrary to what has been asserted, that there is no sensible production of water; there is a condensation of nearly the whole acid and alkaline gasses, nitrogen gas remains, and dry muriate of ammonia is formed. The experiment therefore affords no evidence of the existence of oxigen in oximuriatic acid, but neither is it inconsistent with that hypothesis; for the water, that, in conformity to it, must be supposed to be formed, may exist in combination with either or both of the principles of the compound salt.

or the cases where oxigen is pro ured from oximuriatic acid:

Gay-Lussac and Thenard have proved, it is remarked by Mr. Davy, by a copious collection of instances, that, in the usual cases where oxigen is procured from oximuriatic acid,

acid, water is always present, and muriatic acid is formed; and as oximuriatic acid, he adds, is converted into muriatic acid gas by combining with hidrogen, it is scarcely possible to avoid the conclusion, that the oxigen is derived from the decomposition of water. Such facts however admit equally of explanation on the supposition, that the oxigen is derived from the decomposition of the oximuriatic acid, but that water is necessary to aid this decomposition by the affinity it exerts to the muriatic acid. They are not therefore of themselves more favourable to the one hypothesis than to the other.

Or the action of metals on muriatic acid gas:

When muriatic acid gas is acted on by metals, the acid disappears, and hidrogen gas is produced. In these cases Mr. Davy supposes, that the muriatic acid is decomposed, its oximuriatic acid combines with the metal, and its hidrogen is evolved, and accordingly the compounds formed are the same he remarks as those produced by the direct action of oximuriatic acid on the same metals. But this explanation rests on the previous assumption of the truth of the theory, and the phenomena are equally explained by supposing the hidrogen to be derived from the decomposition of the portion of water combined with the acid in its gaseous state, the oxigen of this water combining with the metal, and the oxide thus formed uniting with the real acid. It is true, that this production of hidrogen takes place even when the muriatic acid gas has been previously exposed to substances having a strong attraction to water, so as to bring it to its driest state; but the same explanation may still be given, for when the acid is in this state it can only be affirmed, that a portion of water has been abstracted from it, not that the whole has been withdrawn. As the water is removed from the acid gas, the affinity by which the remaining quantity is retained must progressively increase in strength, until it equal or exceed that of the substance attracting it; when of course its abstraction must cease. The phenomena then afford no decisive proof of Mr. Davy's hypothesis, for before the explanation given of them in conformity to that hypothesis can be received as exclusively just, it must be established by prior and independent evidence, either that muriatic acid does not contain combined water, or that it is a compound of oximuriatic acid and hidrogen. If

or the inability to extract oxigen from compounds formed by the action of oximuriatic acid gas on inflammables.

If the compounds formed by the action of oximuriatic acid gas on metals, or inflammables, contain oxigen, as they must on the common hypothesis of the constitution of that acid be supposed to do, indications of it may be discovered in the products of their decomposition, and thus evidence may be gained of the existence of oxigen in that acid. With the view of determining this Mr. Davy instituted several experiments. From the substance formed for example by the action of oximuriatic acid gas on tin, he endeavoured to procure oxide of tin by the addition of ammonia; and from that formed by the action of the same acid on phosphorus, he endeavoured by a similar method to obtain phosphoric acid. But in neither case was the experiment successful, the ammonia remaining in the combination, and forming products possessed of very singular properties, and in particular very difficult of decomposition. Still it may be remarked these experiments do not preclude the supposition, that oxigen may have existed in these combinations; for, as the compounds were not decomposed, it cannot be affirmed precisely what is their composition. They afford it is true no proof, that oxigen had been communicated from the oximuriatic acid; but neither do they give any proof, that this had not been the case. They may be compounds of the oxigenated metal or inflammable with muriatic acid and ammonia; and the difficulty of decomposing them, and abstracting the acid so as to discover this oxigenated base, may, in common with all the preceding facts, be ascribed to the peculiar relation of this acid to water, the absence of that portion of it necessary to the constitution of the acid gas preventing its disengagement from the base, precisely in the same manner that the same acid cannot be expelled from the dry muriate of potash or soda by a concrete acid, unless water be supplied.

The singular fact, that perfectly dry charcoal will not decompose oximuriatic acid gas, more in favour of his hypothesis:

There is one fact, which Mr. Davy has established, extremely singular; and which appears more conclusive against the hypothesis, that oximuriatic acid is a compound of muriatic acid and oxigen. If charcoal, freed from hidrogen and moisture by intense ignition in vacuo, be heated even to whiteness in oximuriatic acid gas, it produces no change in it. Now if oximuriatic acid be a compound of oxigen with muriatic

muriatic acid; and especially if, according to the common opinion, this oxigen is retained in combination with the acid by an affinity comparatively weak; charcoal might be supposed capable of abstracting it, at least with as much facility as hidrogen, which there is no reason to believe has a stronger attraction to oxigen. On the common hypothesis there appears no cause why it should not do so; and the fact seems favourable to the theory, that hidrogen causes the disappearance of oximuriatic acid, not by abstracting oxigen from it, but by combining with it, and converting it into muriatic acid. It is this experiment accordingly, Mr. Davy has added, which "led me to doubt of the existence of oxigen in that substance, which has been supposed to contain it above all others in a loose and active state."

I may first remark with regard to this singular fact, that it presents an anomaly just as great according to the new as according to the old hypothesis. Oximuriatic acid is considered in the former as a principle belonging to the same class as oxigen, which like it exerts powerful attractions to inflammable substances, and which is not inferior in energy of action. Why therefore should it not like oxigen combine with charcoal?

But why does not the charcoal combine with this new acidifying principle?

But farther it appears to me, that a solution may be given of this difficulty; that this apparent anomaly is connected with the preceding facts; and that the theory of it is to be derived from the peculiar relation of muriatic acid to water. Admitting that water combines intimately with this acid, and that it is essential to its transition to the elastic form, it is obvious, that, when charcoal is heated in oximuriatic acid gas, although it might attract the oxigen, and combine with it, there is no source whence the portion of water essential to the production of the muriatic acid gas can be supplied; the absence of this water therefore prevents the transition to the state of muriatic acid, and of course prevents the decomposition of the oximuriatic acid, and the consequent oxigenation of the charcoal. It is possible even that this may be aided by a similar necessity, though to a less extent, of the presence of water to the constitution of carbonic acid; as there are facts, which render it probable, that a portion of combined water exists in this

Probably because nothing is present to form the water essential to the constitution of the muriatic acid gas,

and perhaps also of the carbonic acid gas.

this gas, and is necessary to its transition to the elastic form. In treating charcoal in oximuriatic acid gas, all the circumstances are thus unfavourable to the oxigenation; as the products of this, the muriatic acid, and the carbonic acid, both require water to their constitution, while there is no source whence it can be supplied. There is another principle, which also probably operates in giving rise to this apparent anomaly. When a metal is placed in oximuriatic acid gas, its oxigenation is probably the result of that singular kind of chemical action named disposing affinity, being promoted, according to the language usually employed to express this, by the affinity of the muriatic acid to the metal in its oxidated state. The combination of hidrogen with the oxigen of oximuriatic acid is in conformity to the same law probably promoted by the attraction of muriatic acid to water. But this cause does not operate when charcoal is heated in oximuriatic acid gas, for there is no sensible, or at least no forcible attraction exerted between muriatic acid and the product of the oxigenation of charcoal, whether it be carbonic oxide or carbonic acid. Hence too charcoal is not acted on by liquid muriatic acid, while metals are oxidated and dissolved by it.

Want of what is called disposing affinity probably too operates in this case.

Of this phenomenon therefore, apparently so singular, an explanation, which I conceive is satisfactory, may be given, while it remains an anomaly on the opposite theory; for, on the assumption that oximuriatic acid is an acidifying principle having a strong attraction to inflammable bodies, no cause is assigned, why it should not combine with charcoal, as well as with hidrogen, or with metals.

This anomaly not so easily accounted for on Mr. Davy's hypothesis.

It will probably now be apparent, that all these facts are connected with the general question, whether water combines intimately with muriatic acid, and is essential to its constitution in the elastic state. On the assumption that this is true, they are capable of being explained by the common theory of the relation between muriatic and oximuriatic acid; on the denial of this proposition, the theory Mr. Davy has proposed affords another mode in which they are accounted for. But neither of these hypotheses (for as such only can they be regarded) is established by these facts

The question is therefore, whether water be essential to the constitution of muriatic acid gas.

to

to the exclusion of the other, and both, therefore, stand precisely on equal grounds.

Comparing them for a moment under this point of view, the unity and simplicity of the common theory are much in its favour. There is some improbability in the hypothesis of two acidifying principles distinct from each other, but exerting similar chemical agencies. The explanations which this involves of the combinations of these principles with inflammables and metals, and of the relation of water to them, are extremely complicated; while on the more simple system opposed to this, nothing more is required than the assumption, if not proved, at least not improbable, of the existence of a portion of combined water in muriatic acid gas. And instead of any generalization, there is in the formation of the hypothesis, that oximuriatic acid is an acidifying principle, an evident adaptation to the phenomena peculiarly related to this question; since it does not appear, that this principle enters into the composition of any acid but the muriatic, or gives rise to acidity in combining with any substance but hidrogen.

Unity and simplicity of the common theory.

Instead of leaving the subject, however, on these general grounds, it were desirable to gain more conclusive evidence. There are two modes of proof, by which the question may be determined. Either it may be shown, that muriatic acid does, or does not contain combined water; or it may be proved, that oxigen does or does not exist in oximuriatic acid. It is only necessary, that the facts, on which the conclusion with regard to these propositions is to rest, should not be capable of being accounted for by the one hypothesis as well as by the other; and to attain this is extremely difficult from the facility with which both can be adapted to the phenomena.

Modes of deciding the question.

Thus no proof would appear at first view more decisive of the existence of combined water in muriatic acid gas, and the necessity of this to the transition of the acid to the elastic form, than that afforded by the fact established by the experiments both of Gay-Lussac and Thenard, and of Mr. Davy—that if a dry muriate, muriate of potash for example, be heated with a concrete acid, as the phosphoric or boracic, it is not decomposed, nor is any muriatic acid gas disen-

Argument from water being necessary to the decomposition of a muriate by an acid inconclusive.

disengaged; but if a little water be added, very abundant vapours of the acid immediately arise. The obvious conclusion from this is, that water is essential to the constitution of the muriatic acid gas. Yet this is avoided by supposing in conformity to Mr. Davy's hypothesis, that dry muriate of potash is not a compound of muriatic acid and potash, but of oximuriatic acid and potassium; and farther supposing, that the water added is decomposed, its hidrogen combining with the oximuriatic acid, and converting it into muriatic acid, and its oxigen changing the potassium into potash.

Indirect argument for the existence of water in the driest muriatic acid gas.

There are some indirect considerations, however, from which the existence of combined water in muriatic acid gas may be inferred, or which at least give probability to such a supposition. Thus it is obvious from the facility with which this acid when in the gaseous state is condensed by water, and from the large quantity of it that is absorbed, that an affinity of considerable strength exifts between them. In consequence of this affinity the acid in its gaseous state must retain a portion of water combined with it; and from the law of chemical attraction so well established, that quantity adds to its force, it follows, that this water will be retained in combination with a strength of attraction progressively increasing as it is abstracted; and that a portion, therefore, must probably remain, in the driest state to which muriatic acid gas can be brought.

The probability of this strengthened by the analogy of carbonic acid gas.

The supposition, that muriatic acid contains combined water, and that this is essential to its gaseous state, is rendered more probable by this influence of the combination of water being established with regard to other elastic fluids, where there is not the same ambiguity. Carbonic acid cannot be easily expelled by heat, or even by the action of an acid, from native carbonate of barytes; while it is expelled from the artificial carbonate with facility; though the only difference between the carbonate in these forms is, that in the one it contains a portion of water, and in the other it appears to contain none. And accordingly, if water be applied to the native carbonate, it favours its decomposition, and the expulsion of the carbonic acid. In this case is clearly established the influence of water with regard

regard to carbonic acid, and its transition to the elastic state; and this cannot be explained by any hypothesis similar to that, by which the analogous influence of water with regard to muriatic acid is attempted to be explained. It renders more probable, therefore, the explanation in conformity to the common theory with regard to this acid inferred from similar facts. It may be added, that the affinity of muriatic acid to water being evidently stronger than that of carbonic acid, the former in its gaseous state may be presumed to retain a larger quantity combined with it than the latter.

Proof of the existence of oxigen in oximuriatic acid not easily obtained.

The other mode of proof, whence this question may be determined, is that relating to the existence of oxigen in oximuriatic acid. The facts from which this has usually been inferred, either by the apparent formation or decomposition of the acid, it has already been remarked are ambiguous, as they admit equally of explanation on Mr. Davy's hypothesis. The object is to obtain proof not liable to this ambiguity, and one train of investigation occurred to me, whence I imagined such proof might be obtained.

Decomposition of the acid by charcoal with the addition of water gives imperfect or ambiguous results.

The cause which I have assigned for charcoal not being oxigenated by oximuriatic acid is, that the portion of water necessary to the constitution of muriatic acid gas is not afforded. If water, therefore, were supplied, the oxigenation ought to take place. There is some reason to believe that this is the result, but from the state of aggregation of the charcoal, it is not easy to conduct the experiment, so as that it shall not be either imperfect or ambiguous.

Argument from oximuriatic acid and carburetted hidrogen forming muriatic and carbonic acids.

But there is another form under which it may be made, and with regard to which indeed the facts are already partly ascertained. If oximuriatic acid gas, and any of the varieties of what are named carburetted hidrogen gasses be mingled together, it is known from the experiments of Cruickshank, that a mutual action is exerted, either immediately if the electric spark is taken in the mixture, or more slowly if they are allowed to remain in their mixed state; and in either case, if the due proportions are observed, they are entirely converted into muriatic and carbonic acids. The explanation of this according to the common theory is obvious; the carbon and hidrogen of the inflammable gas receiving oxigen from the oximuriatic acid, carbonic acid and water

water are formed, and this water must be supposed to be retained in combination with the muriatic acid, into which the oximuriatic is converted. But it does not admit equally of explanation on Mr. Davy's hypothesis. The oximuriatic acid may be supposed indeed to combine with the hidrogen of the carburetted hidrogen to form muriatic acid. But whence is the oxigen derived, which converts the carbon into carbonic acid? The result, in strict conformity to the hypothesis, ought to be the conversion of the oximuriatic acid into muriatic acid, and the deposition of the carbon in a state analagous to charcoal. This is indeed the result when some of these gasses are acted on by a certain quantity of oximuriatic acid; but when the quantity is larger, the product is uniformly carbonic acid.

Only apparent source of ambiguity

There appears to me to be only one source of ambiguity attending this experiment, and that probably a very trivial one. The inflammable gasses, which have been regarded as binary compounds of carbon and hidrogen, are probably rather ternary compounds of carbon, hidrogen, and oxigen; and on this supposition it may be said, that, while the oximuriatic acid combines with the hidrogen, and forms muriatic acid, the carbon and oxigen of the inflammable gas remain in union, and form carbonic acid. This explanation however implies the existence of a much larger quantity of oxigen, than there are any grounds for supposing does enter into the composition of these gasses. It besides fails to account for the large quantity of oximuriatic acid that is required. Little more than one measure of oximuriatic acid gas to one measure of hidrogen is required for mutual saturation, so that the whole is converted into muriatic acid gas. But not less than four measures of oximuriatic acid gas to one measure of carburetted hidrogen are necessary to produce the full change into carbonic acid; and if less be employed, there is either a deposition of charcoal, or a formation of carbonic oxide. Now this quantity cannot be required merely for the saturation of the hidrogen contained in the carburetted hidrogen, and what other agency can it be supposed to exert, by which it produces the conversion of the car-

answered.

bon

bon into carbonic acid, than that of communicating oxigen?

The experiment however it occurred to me might be made in a manner free even from this ambiguity. Cruickshank observed, that, when carbonic oxide is mixed with oximuriatic acid gas, on taking the electric spark in the mixture there is no detonation produced, nor any sensible change. This must be ascribed, on the common hypothesis, to the same cause as the nonoxigenation of charcoal in oximuriatic acid gas—the absence of that portion of water, which is essential to the constitution of muriatic acid gas. But he observed, that, if the two gasses were mixed together, a mutual action is slowly exerted, and after twenty four hours they are converted into carbonic and muriatic acids. Now this is a result different from what the view of the subject I have given would lead to expect, and apparently too in opposition to the result obtained when the action of the electric spark has been employed; for there is no obvious cause why the oxigenation of the carbonic oxide should be effected in the one case, more than in the other. This difference it appeared probable depended on the agency of water. When the electric spark is taken in the mixture of the gasses, even though they may not have been previously dried, the quantity of water within the sphere of agency of the spark is so inconsiderable, that the action is not favoured. But in the slow action the whole water diffused through them can operate successively, and therefore with more effect, so as to admit of at least a partial oxigenation, and if the gasses have been passed through water into the bottle in which they are allowed to remain mixed, (the manner probably in which the experiment was performed by Cruickshank) the film of water adhering to the internal surface may render the action complete. To ascertain if this supposition were just, I made the experiment in the following manner.

Carbonic oxide and oximuriatic acid gasses not changed by the electric spark,

though they are by standing together.

This difference owing probably to the presence of water.

Two measures of carbonic oxide gas, and two measures and a half of oximuriatic acid, each having been previously exposed to muriate of lime, were mixed together. The mixture could not be passed through quicksilver without the quicksilver being acted on by the acid, so that a portion

Experiment to ascertain this. The two gasses made as dry as possible stood together 24 hours without

being decomposed; tion of it adhered to the sides of the jar, and continuing to act on the acid might counteract any action on the carbonic oxide. To avoid this, the mixed gasses were introduced into a caoutchouc bottle connected by a tube and stop-cock with a jar containing dry mercury. On turning the stop-cock, and suddenly compressing the elastic bottle, the gas descended into the jar, and depressed the mercury without acting upon it, this action being prevented still more effectually by a little pure carbonic oxide having been previously introduced into the jar. The stop-cock being shut, and the jar being also closed at the bottom, the mixed gasses were allowed to stand 24 hours. At the end of this time the green tinge of the oximuriatic acid gas was perfectly apparent; the stop-cock being turned under quicksilver, a very slight diminution of volume, not exceeding $\frac{1}{10}$ of the whole mixture was indicated by the entrance of a portion of quicksilver equivalent to this. On transferring to water, there was no immediate absorption, but the oximuriatic acid gas was gradually condensed. The residual gas was then transmitted through lime water in a long narrow tube, it communicated a milkiness to the surface, so slight as to be barely perceptible; this was removed on agitation, and did not appear on a second or third transmission. The residual air was found to be carbonic oxide, both by the test of oximuriatic acid aided by water, and by being converted by combustion into carbonic acid. In this experiment then the carbonic oxide had scarcely been acted on by the oximuriatic acid, and the very minute quantity of carbonic acid that appeared to be formed must be regarded as owing its origin to the impracticability of abstracting water entirely from the gasses, or to the presence of a small proportion of hidrogen in the carbonic oxide.

but they were decomposed in this time, if a little water were admitted, or they were merely passed through water. I now mixed carbonic oxide and oximuriatic acid in the same proportions as in the preceding experiment, and allowed a little water to remain in the bottle. On opening it under water after 24 hours, the carbonic oxide was found to be converted into carbonic acid, and the oximuriatic acid into muriatic acid, so that after agitation with lime water to absorb the former, scarcely any sensible quantity of elastic fluid remained. The same result was obtained when the gasses were merely

merely transmitted through water into the bottle, the small quantity of water diffused through them, and adhering to the glass, being sufficient to promote their mutual action.

The rationale of both experiments is obvious. The circumstance of there being no mutual action in the first experiment must be ascribed to the absence of that water essential to the constitution of muriatic acid. In the second experiment this is supplied; hence the transfer of the oxigen from the oximuriatic acid, and the consequent formation of muriatic and carbonic acids. But they do not appear to admit of an equally satisfactory explanation on Mr. Davy's hypothesis. For the peculiarity that carbonic oxide suffers no change in the first experiment no cause can be assigned, but that oximuriatic acid has no tendency to combine with carbonic oxide or its base; and this is an evident anomaly on the assumption, that oximuriatic acid is a principle exerting like oxigen powerful affinities to inflammable bodies. To account for the agency of the water in the second experiment it must be supposed, that it suffers decomposition, affording hidrogen to the oximuriatic acid, and oxigen to the carbonic oxide. But there are no just grounds, whence this can be inferred. Water is not decomposed by oximuriatic acid gas, or by carbonic oxide gas; there is no reason to conclude, that its decomposition can be effected by their action when they are presented to it merely in a mixed state; and the more obvious operation may be regarded as the real one, that it acts by its affinity to muriatic acid.

This readily accounted for by the common theory,

but not by Mr. Davy's.

But even this source of ambiguity I supposed might be obviated by making the experiment under another form. I mixed together one measure of carbonic oxide gas, one measure of hidrogen gas, and two measures and a half of oximuriatic acid gas, each previously having been exposed to muriate of lime, and introduced them into a jar in the same manner as in the first experiment. The green tinge of the oximuriatic acid gas gradually disappeared, and after two hours exposure to clear day light was not apparent. On opening the jar after 24 hours under quicksilver, a diminution of volume, less as the gasses had been completely dried, was indicated

Experiment tending farther to obviate the ambiguity.

Hidrogen mixed with the dry gasses promoted their decomposition,

dicated. On transferring it into water a farther diminution took place, the water filling rather more than half the jar. On passing the gas through lime water it communicated to it an abundant milkiness, and this repeatedly. A small portion of air remained, which on approaching a lighted taper to the orifice of the tube exhibited a weak flame.

though there was no source of oxigen to form the carbonic acid, on Mr. Davy's hypothesis.

In this experiment then the greater part of the carbonic oxide had been converted into carbonic acid. On repeating it with rather a larger proportion of oximuriatic acid, three measures and a half to one measure of carbonic oxide and one of hidrogen, the changes were similar, but even more perfect, the residual air being scarcely, if at all, inflammable. This result appears to be conclusive in proof of the existence of oxigen in oximuriatic acid, and to be free from the ambiguity which attends the experiment where water is employed to favour the action of the gasses. Hydrogen only is supplied, and how is its agency to be explained? On the one theory it may be conceived to form water by attracting oxigen from a portion of the oximuriatic acid, and this, favouring its transition into muriatic acid, promotes its farther decomposition, so as to afford oxigen likewise to the carbonic oxide, and convert it into carbonic acid. But in the opposite theory, although it may be assumed that the hidrogen will convert the oximuriatic into muriatic acid, what is to produce the oxigenation of the carbonic oxide?

A mixture of carbonic oxide and oximuriatic acid gas exposed to the electric spark for 15 min.

The carbonic oxide remained unchanged.

Cruickshank had found, that no explosion is produced by the electric spark in a mixture of carbonic oxide and oximuriatic acid gasses. To confirm the preceding results, I repeated this experiment with a due attention to the object I had in view; one measure of dry carbonic oxide, and two measures of oximuriatic gas being subjected to the action of the electric spark over quicksilver for fifteen minutes. No detonation was produced, nor any sensible effect, farther than a diminution of volume to the extent of half a measure from the action of the quicksilver on the oximuriatic acid. On removing the excess of oximuriatic acid gas, by water, the residual air occupied almost exactly the volume of one measure; when transmitted through lime water it did not render it milky; it burned with

with the blue flame of carbonic oxide, and was converted into carbonic acid. In this experiment then the carbonic oxide had evidently sustained no change.

I now put into the tube one measure of dry carbonic oxide gas, one measure of dry hidrogen gas, and three measures of oximuriatic acid gas. On transmitting the electric spark there was an instant explosion with the production of a whitish vapour, and diminution of volume. On removing the tube into water the vapour was condensed, a slight tinge of oximuriatic acid remained, but this was soon removed with farther diminution of volume by the action of the water. The residual air occupied scarcely one measure, and by the test of lime water was found to be carbonic acid with a small portion of atmospheric air. A similar result without any remaining excess of oximuriatic acid was obtained on using rather a larger proportion of hydrogen.

A similar mixture, with the addition of hidrogen, exploded, and the gasses decomposed.

Here the ultimate results are the same as in the experiments on the slow mutual action of these gasses, and the same explanation is to be given. There is the same difficulty too in accounting for them by Mr. Davy's hypothesis; for although the hidrogen might convert a portion of the oximuriatic acid into muriatic acid, there is no source whence the oxigen, which has converted the carbonic oxide into carbonic acid, can have been obtained; and the results according to that hypothesis ought to be merely the disappearance of the hidrogen, and the formation of a portion of muriatic acid, while the carbonic oxide with about half of the oximuriatic acid remained unchanged.

This confirms the preceding explanation.

There is only one mode, in which the force of this conclusion drawn from the results of the experiments in which hidrogen is employed, whether in the slow action, or in the detonation by electricity, can be attempted to be obviated. It is that of assuming, that the carbonic oxide is not converted into carbonic acid previous to the admission of water, that it forms with the hidrogen and oximuriatic acid a ternary compound which decomposes the water, and attracts its oxigen and hidrogen, so that the carbonic oxide becomes carbonic acid, and the oximuriatic, muriatic acid. But this is a supposition, which, were it advanced, would be altogether gratuitous, and had recourse to merely to avoid a difficulty;

Objection to the conclusions drawn, gratuitous,

difficulty; for there is no evidence of the formation of such a compound from any phenomena, which attend the experiment. This objection too was in a great measure obviated by introducing into the electric fluid, previous to the admission of water, a slip of dry litmus paper: it instantly received a bright red tinge, a proof of the production of acid.

and in great measure obviated.

The experiments often carefully repeated with similar results.

In making these experiments I was assisted by my friend Mr. Ellis; they were often repeated, and every attention was paid to ensure accuracy. The carbonic oxide was obtained first from dry chalk and zinc filings; and lest this should contain a little hidrogen, which might be a source of errour, more particularly in the experiment of the slow action of the gasses, it was afterward prepared from native carbonate of barytes and iron filings. The oximuriatic acid was prepared from muriatic acid and oximuriate of potash. The influence of light in promoting the action of the oximuriatic acid on the inflammable gasses was strictly attended to, the mixture in the different experiments being always exposed for the same length of time to the clear light of day.

Hence we have sufficient evidence of the existence of oxigen in oximuriatic acid.

From these experiments there appears to be sufficient evidence of the existence of oxigen in oximuriatic acid; and I may remark, that carbonic oxide is peculiarly calculated to afford this evidence without that ambiguity, which attends the action of oximuriatic acid on other inflammable substances, as in these the substance which is formed enters into combination with the muriatic acid, and the nature of these compounds becomes the subject of hypothesis. In the action of oximuriatic acid on potassium, for example, a substance is formed, which according to the common theory is a compound of oxide of potassium (potash) with muriatic acid; but which, according to the opposite hypothesis, is a compound of oximuriatic acid and potassium: and it is not easy to submit it to any experiment, from which it may be determined which of these views is just. But in the case of carbonic oxide, the muriatic acid, having no great attraction to the product, does not combine with it; its nature therfore is discovered, and the obvious production of it from the carbonic oxide proves, that oxigen must have been communicated, and that it could be so only from the oximuriatic acid.

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In reflecting on other cases of chemical action exerted by oximuriatic acid on inflammable substances, there appeared to be one, in which nearly the same advantage might be expected. From its action on sulphur a compound is formed, which, in conformity to Mr. Davy's hypothesis, is composed of oximuriatic acid and sulphur; but which, according to the other, consists of sulphur in some state of oxidation, and muriatic acid, free or nearly so from water. But when sulphuretted hidrogen is acted on, a different result may be expected; the hidrogen receiving oxigen from the oximuriatic acid will form water, which favouring the production of muriatic acid, oxigen will likewise be communicated to the sulphur, and convert it into sulphurous or sulphuric acid; to either of which the muriatic acid exerting no strong attraction, their production will be apparent. When oximuriatic acid gas and sulphuretted hidrogen gas are mixed together over water, it is known, that a rapid diminution of volume takes place, and sulphur is deposited. Mr. Davy has stated the results more minutely, excluding the agency of water. The two gasses, previously dried, were mixed in equal volumes; the condensation was not $\frac{1}{40}$, sulphur which seemed to contain a little oximuriatic acid was formed on the sides of the vessel, no vapour was deposited, and the residual gas contained $\frac{1}{20}$ of muriatic acid, the remainder being inflammable.

Farther evidence from the action of oximuriatic acid gas on sulphur, and on sulphuretted hidrogen.

This result appeared to me to arise probably from the proportions of the two gasses, the quantity of oximuriatic acid not being sufficient to afford oxigen to convert both the hidrogen into water, and the sulphur into sulphuric acid; and the hidrogen suffering the oxigenation most rapidly, so that the sulphur is deposited without being acted on. If a sufficiently large proportion were employed, it seems to follow from the preceding reasoning, that the oxigenation of the sulphur to a certain extent at least ought to take place, the formation of water from the oxigenation of the hidrogen favouring the transition of the oximuriatic to muriatic acid. Proceeding on this principle I obtained the following results from experiments varied with regard to the proportions. When one measure of oximuriatic acid gas was mixed over water with two measures of sulphuretted hidrogen,

Different results from different proportions of the gasses.

1 part oximuriatic gas and 2 sulphuretted hidrogen.

hidrogen, copious dense fumes were produced with a deposition of sulphur on the sides of the jar, and on the surface of the water rendering it turbid. When these proportions were reversed, there was also the production of a dense vapour and a deposit of sulphur; but the water, which was at first rendered turbid, soon became transparent as it rose in the vessel, the film of sulphur disappeared, and the entire gas nearly was condensed. When oximuriatic acid gas was allowed to escape from a retort into sulphuretted hidrogen gas in a jar, a very dense vapour was produced as each bubble of gas broke from the surface of the water, sulphur was collected on the surface, and the water was quite turbid. When the experiment was reversed, the oximuriatic acid gas being in the jar, and the sulphuretted hidrogen allowed to escape into it, a whitish vapour was also produced as each bubble of gas broke, but much less dense and less in quantity, and there was scarcely any deposition of sulphur; obviously owing to this, that the sulphuretted hidrogen from its inferior specific gravity rapidly diffused itself through the mass of oximuriatic acid gas, which thus acting upon it in large quantity oxigenated both the hidrogen and the sulphur, while in the other mode the reverse of this happened*.

1 oxim. acid, 2 sulph. hidrogen.

Oxim. acid gas received in sulph. hidr.

Sulph. hidr. received in oxim. acid gas.

Experiments with these gasses repeated in different forms.

To exclude the agency of water, one measure of sulphuretted hidrogen was admitted into an exhausted tube, and afterward three measures of oximuriatic acid gas. A whitish vapour immediately spread through the tube, but it was not very dense, nor was there any deposition of sulphur;

Singular phenomenon.

* A singular appearance presented itself in these experiments. As each bubble of oximuriatic acid gas broke in the sulphuretted hidrogen, there was an explosion with a vivid flash of light, though no evident combustion attends the direct mixture of the two gasses, and there was no similar appearance when the sulphuretted hidrogen broke in successive bubbles in the oximuriatic acid gas. The difference may be owing principally to the influence of the specific gravity of the gasses as above explained, the oximuriatic acid, when introduced into the sulphuretted hidrogen, from its greater density not diffusing itself, and therefore acting in a more concentrated state; partly also perhaps to a slight change produced on oximuriatic acid by transmission through water.

and

and the air in a few minutes regained its transparency. On introducing the gasses in equal volumes into the tube a very dense vapour was produced, with a copious deposition of sulphur. The experiment was repeated in another form. Oximuriatic acid gas was introduced into a glass globe with a stopcock, and expanded by heat so as to produce a partial exhaustion. On admitting sulphuretted hidrogen in small successive portions, a very slight vapour was at first produced, and it soon disappeared; but on the subsequent admissions it became more and more dense. Part of the residual air, after having regained its transparency, was expelled from the globe by heat; and on admitting a fresh portion of sulphuretted hidrogen, there was a copious deposition of sulphur on the sides of the globe. This however after some hours had in a great measure disappeared. On turning the stopcock under water, there appeared to be little diminution of volume, a small portion of water only entering the narrow tube of the stopcock; but when this was diffused through the globe by agitation, on again opening it water rushed in so as nearly to fill it. It had therefore been filled with muriatic acid gas which the water instantly condensed; and as there was little deposition of sulphur it may be concluded, that the greater part of it had been converted into sulphuric, or partly perhaps into sulphurous acid. The water was acid and gave a precipitate with muriate of barytes. The results of these experiments thus sufficiently accord with the supposition, that, when sulphuretted hidrogen is acted on by oximuriatic acid, the sulphur receives oxigen when the due proportion of acid is employed.

All the facts connected with the subject of the preceding researches appear in the present state of the investigation to lead to the adoption of one of two general systems, each of which involves several subordinate propositions necessarily connected. In the one—that proposed by Mr. Davy, oximuriatic acid is regarded as a simple substance, and muriatic acid as a compound of this substance with hidrogen. In the other—oximuriatic acid is held to be a compound of muriatic acid and oxigen; the composition of muriatic acid itself is unknown, but it is supposed to have a peculiar relation

General observations.

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tion to water in consequence of which they combine intimately, this combined water powerfully modifying the affinities the acid exerts. In the former system oximuriatic acid is supposed to form an extensive series of compounds with inflammable and metallic bases; muriatic acid forms another order of compounds with these bases in an oxidated state; and the former of these compounds are converted into the latter, when water is communicated to them, by the water being decomposed, its oxigen being transferred to the base, and its hidrogen to the oximuriatic acid. In the latter hypothesis the facts connected with these combinations are explained by supposing the one order of compounds to be composed of muriatic acid free from water combined with inflammables and metals in an oxidated state: the difficulty of decomposition, the character by which they are distinguished from the common muriates must be abscribed to the absence of the water essential to the existence of muriatic acid in an insulated form; and the changes they suffer from the communication of water must be supposed to arise from the changes it produces in the chemical powers of the acid. In the latter hypothesis the only difficulty which appears to me of importance is that arising from this peculiar relation between muriatic acid and water. It undoubtedly presents an anomaly in chemical theory; for although there is no difficulty in supposing from the evident affinity which exists between these bodies, that the acid in its elastic form may hold a portion of water combined with it, it is not obvious by what agency this water should be so essential to its existence in this form, or should have so important an influence on its chemical powers. All the other phenomena however appear to admit of explanation on this hypothesis, and if the conclusions I have ventured to draw from the preceding experiments be just, it is supported by more direct evidence. The peculiarity with regard to water must therefore be elucidated by farther investigation, and it presents an interesting subject of research.

Only anomaly on the old theory,

which presents an interesting subject of research.

I have the honour to be,

Sir, yours, &c.

J. MURRAY.

Questions

IX.

Questions on the Nature of Water. In a Letter from a Correspondent.

To Mr. NICHOLSON.

SIR,

WILL you permit a young Tyro in chemistry, to solicit information through the medium of your Journal, upon a few observations of some of your celebrated chemists, which have excited his attention, and unsettled his ideas.

In the last edition of Mr. Parkes's Chemical Catechism, p. 106, is the following note. "Some recent speculations "of Mr. Davy seem to involve the conclusion, that water is not a compound body, but the ponderable base "both of *oxigen* and *hidrogen* gasses, assuming either of "these forms according to its electrical states. See "Murray's Chemistry 2d ed. vol. II, 136; or Mr. Davy's "paper in the Phil. Trans. for 1808."—Now there is not in Dalton's New Chemical Philosophy, or in the 6th edition of Dr. Henry's Elements of Experimental Chemistry (both very recently published) any mention of any such an idea;—the latter considers *hidrogen* as a metallic body, in a state of *opposite electricity* to *oxigen*; for, like other inflammable substances, it is naturally in a state of *positive* electricity, and he distinctly says, that the *two gasses* have *different bases, oxigen* and *hidrogen*; and that water is a compound, not of the two gasses, but of oxigen and hidrogen. Questions on the nature of water.

In the 1st vol. of Dr. Henry's Elements, p. 223, last edition, he says, "When friction is applied to the glass "cylinder of an electrical machine, the electric fluid flows "to it from surrounding bodies, and thence passes to the "prime conductor, in which it exists in a greater than "natural quantity. All then that is effected, by the "action of the machine, is a disturbance of the natural "quantity of electricity in bodies, or a transfer of it from "some to others, in consequence of which, while the latter "acquire a *redundance*, the former become proportionally "*deficient* in this quantity of electricity." And he adds, in a note, "This theory appears to me to have several ad-

"vantages

Questions on the nature of water.

"vantages over the hypothesis, which supposes *two different* "*kinds of electricity*, capable by combination of neutra- "lizing each other. The only modification, I would propose, "is a slight change of terms; and the adoption of thofe "suggested by Mr. Cuthbertson, viz. *condensed* and *rare-* "*fied*, instead of *positive* and *negative*, electricity"—and yet Dr. Henry, not only in the instance before quoted, but in the subsequent parts of his valuable work, upon this subject, observes, throughout, the distinction of *positive* and *negative* electricity*.

In page 173 of Mr. Parkes's book is a note, in which he insinuates, that, as Mr. Davy has discovered that what is termed *oxigen* is the principle of *alkalescence*, as well as of *acidity*, the term *oxigen* is improper, and that *hidrogen* would be a more unobjectionable term.

If any of your correspondents will favour me with remarks upon these points, I shall feel myself under great obligations to them.

I am, Sir,

Your constant reader,

A. B.

Cheltenham,
January 5, 1811.

REMARKS.

The hypothesis, that water constitues the base of oxigen and hidrogen, is by no means new. It was thus that Dr. Priestley, and others who maintained the doctrine of phlogiston, accounted for the appearance of water in Mr. Cavendish's experiment of the combustion of hidrogen with oxigen. Some have even considered water as the ponderable base of all the gasses. The opinion, however, that water is a compound of oxigen and hidrogen, has long been generally admitted; and though Mr. Davy observed, in the paper alluded to, that his experiments at that time seemed in some degree favourable to a modified phlogistic theory, yet a constant reader must have seen, that he considered this opinion by no means supported by his subsequent experiments. C.

* When I wrote the foregoing, I had not seen the article in your last Journal on the electric effects of friction.

X. *Remarks*

X.

Remarks on Mr. La Place's *Theory of Capillary Action. In a Letter from* Thomas Knight, *Esq.*

To Mr. NICHOLSON.

SIR,

Among the objections that may be made to Mr. La Place's theory of capillary action, there is one, that I should be glad to take notice of, through the medium of your valuable publication. Objection to La Place.

The errour alluded to (as well as many others) I have indeed pointed out, in an "Examination of Mr. La Place's Theory," published about a year ago: but it appears, from the present number of the Monthly Review, that what I very clearly expressed has been totally misrepresented. *I hope* it has been *misunderstood**.

It is convenient, in Mr. La Place's view of the subject, to consider, *separately*, the action of a concave meniscus, and that of a fluid mass with a plane surface, on a slender column of fluid. Action of a meniscus and of a fluid with a plane surface considered separately:

This author shows, in a satisfactory and ingenious manner, that, by the action of the meniscus, the column is urged upwards. Now, the same mode of reasoning that is applicable to a meniscus, must also be applicable to a mass of any other form. That very *same* reasoning ought, therefore, to have been employed in determining the action of the mass with a plane surface: and thus it would have been found, that by *this also* the column is urged *upwards*. but the same mode of reasoning applicable to a mass of any form;

Instead of this, Mr. La Place, by a process of quite a different kind (which the Monthly Reviewer esteems vastly simple and ingenious), attempts to show, that this mass with a plane surface urges the column *downwards*. though Mr. L. endeavours to show the contrary.

The falsehood of which may be proved in a moment; for, if we consider any point in the mass, it is easy to see that it will urge the column neither way, or will urge it upwards: and this, by Mr. La Place's own reasoning which he had used when treating of the meniscus. His errour shown.

* The author passes over every material argument in the examination; and has the prudence not to defend Mr. La Place's general theory.

I affirmed,

I affirmed therefore before, and I affirm with the same "*boldness*" again, that, by employing reasonings *totally opposite* in cases *exactly similar*, Mr. La Place and Mr. Haüy did virtually contradict in one passage what they had said in another.

It is asserted by the Monthly Reviewer, in the course of his *pleasant* observations, that, "If they had been guided "by conceptions as vague and uncertain as those of their "examiner, they would to this time have been wandering "in errour."

His results true, from a compensation of errours.

I will show briefly, that, if they had proceeded according to those conceptions, they would have obtained the *same results*, though in a manner somewhat different; consistently, and without arriving at truth by a compensation of errours. Instead of making the mass with a plane surface urge the column *downwards*, with a force K, this letter would have represented its action *upwards*, which added to the action of the concave meniscus, in the same direction, would have given the whole force, with which the column is urged *upwards*, $= K + \frac{H}{b}$*. So that, from this method, there would have arisen ultimately, by the principle of a canal, the equation

$$K + \frac{H}{2}\left(\frac{1}{R} + \frac{1}{R'}\right) - gz = K + \frac{H}{2}\left(\frac{1}{b} + \frac{1}{b'}\right)$$

which, by transposition, is the same as the one now given by La Place, (in p. 19 of his work) and must necessarily lead to the same results.

To conclude—If Mr. La Place had found his fundamental equation in the manner I have pointed out; and, instead of making the whole column support itself, had *truly* shown how it was held up by the tube: there would not, I think, have been any thing to object to his theory †.

I am, Sir,

Your most obedient servant,

Papcastle, Dec. 8th, 1810. THO. KNIGHT.

* Mr. La Place makes it to be urged *downwards* by a force $= K - \frac{H}{b}$, where I use his notation.

† But *then* it would have been a *different* theory.

XI,

XI.

On Muriatic and Oximuriatic Acid, in Answer to Justus. In a Letter from Mr. JOHN DALTON.

To Mr. NICHOLSON.

SIR,

YOUR correspondent who signs himself *Justus* in your last number seems to possess considerable acquaintance with the subjects of his animadversion; but in regard to oximuriatic and muriatic acid at page 72, his data are defective, and the principles of his calculations are to me altogether unintelligible. If he will explain the manner of his calculation, I shall probably on some future occasion reply to his different inquiries and observations. On muriatic and oximuriatic acid.

I remain, yours,

J. DALTON.

Manchester, Jan. 20th, 1811.

XII.

An Analysis of Fluor-Spar. By THOMAS THOMSON, M. D. F. R. S. E*.

THE mineral called fluor-spar has been long known, and valued on account of its beauty, and the ease with which it can be turned on the lathe into various ornaments and useful utensils. It occurs chiefly in veins, and very frequently accompanies lead ore. Some of its properties have been described more than a century ago, as, for example, its phosphorescing when heated, and its corroding glass when mixed with sulphuric or nitric acid. But it is not forty years since its composition was discovered by Scheele, who demonstrated, that it is composed of lime, and a peculiar acid, called fluoric. Chemists now distinguish it by the name of fluate of lime. Fluor spar, or fluate of lime.

Hitherto, no chemical analysis of this salt has been published, except a very incorrect one by Kirwan and Gren, which has been ascribed to Scheele, though I cannot find it in any of his dissertations on fluor-spar. By that analysis, it is made to contain 27 *per cent* of water, a proportion very inconsistent with the properties of native fluate of lime, No accurate analysis of it yet published.

* From the Transactions of the Wernerian Society.

which

which, when strongly heated in a wind furnace, loses at an average only $\frac{1}{600}$th part of its weight. The obvious inaccuracy of the analysis given by the authors just mentioned induced me, to make a set of experiments on it last summer (1807). I selected the purest, transparent, colourless crystal, which I found by repeated trials to be very nearly pure fluate of lime. When reduced to a fine powder, and digested in nitric acid, I found in the acid only a little lime, owing doubtless to the partial decomposition of the fluate, and minute traces of iron and lead: these two metals I detected, by evaporating the nitric solution to dryness, heating it to redness, and then dissolving the residue in muriatic acid. The colour of the solution showed the presence of iron, and a few needleform crystals of muriate of lead were deposited after the solution had stood for some days. The fluate, which I used, had been dug out of a lead mine in Northumberland, and small crystals of sulphuret of lead were here and there to be seen in it. Probably some one of these had escaped my attention, and, by being mixed with the fluate which I used, occasioned the appearance of the lead, which, however, did not amount to $\frac{1}{300}$th part of the salt, and therefore could not occasion any sensible errour in the subsequent analysis.

Experiments on it.

Digested in nitric acid.

I first tried to decompose the fluate of lime, by fusing it with twice its weight of carbonate of potash in a platinum crucible. Only a small portion of the fluate was decomposed. I was, therefore, obliged to repeat the fusions very often, washing off the alkali after each operation by means of water, and then dissolving the carbonate of lime formed, in muriatic acid. Fatigued with the tediousness of this method, and despairing of an accurate result from the great number of successive solutions, I abandoned it altogether, and adopted the following method, much more expeditious and equally precise.

Fused repeatedly with carbonate of potash.

From a mass of fluor spar, which I had ascertained to contain no sensible portion of foreign matter, I separated 100 grains, which I reduced to powder, and digested for some hours in a platinum crucible, with rather more than an ounce of pure concentrated sulphuric acid. The mixture was then evaporated to dryness, and the crucible exposed

100 grains digested in sulphuric acid,

posed for an hour to a strong heat, in a wind furnace. To ensure the complete decomposition of the fluor, the mass was reduced to powder, and treated a second time in the same manner with another ounce of sulphuric acid. The residue, which was white with a slight shade of red, proved on examination, to be pure sulphate of lime. It weighed 156·6 grains.

gave 156 6 of sulphate of lime,

Now, it has been ascertained by the most careful experiments, that sulphate of lime, thus violently heated, contains 43 per cent of lime. Of consequence, the whole quantity of lime in 156·6 grains of sulphate is 67·34 grains. This is obviously the whole lime contained in 100 grains of fluor spar; and since that mineral contains no sensible portion of water, the remainder of the 100 must be fluoric acid. Hence fluate of lime is composed of,

consequently contained 67·34 grs of lime,

Lime	67.34
Fluoric acid	32·66
	100·00

and 32·66 fluoric acid.

This result differs very materially from the analysis alluded to in the beginning of this paper, which makes the amount of the acid in the same weight of spar, only 16 grains, and the lime 57 grains; the remaining 27 grains being considered as water. But from the care with which my experiment was conducted, I flatter myfelf, that the result which it exhibits is very near the truth*.

SCIENTIFIC NEWS

Mr. George Singer's course of Lectures on Electricity and Electrochemistry will commence on the 7th of February, at the Scientific Institution, Prince's Street, Cavendish Square. They embrace not only all that has formerly been done, but the most recent discoveries of Mr. Davy, and Mr. De Luc's analysis of the Voltaic pile, an account of which has appeared in the present and two preceding volumes of our Journal.

History and practice of electricity.

St. Thomas's and Guy's Hospitals.

The Spring Courses of Lectures at these adjoining Hospitals will commence the beginning of February, as usual.

Medical Lectures.

TO CORRESPONDENTS.

Mr. Moore's and Mr. Gregor's communications were too late for the present number, but will appear in our next; as will Mrs. Ibbetson's on the corollas of flowers.

* Since the preceding paper was read, I have seen an analysis of fluor spar by Klaproth, in the 4th volume of his Beiträge, conducted in a very different manner from mine, but leading to almost the very same result. He found fluate of lime composed of $67\frac{3}{4}$ lime, and $32\frac{1}{4}$ acid — quantities which coincide with mine, within less than 1 per cent.—— Nov 1809.

The result agrees with Klaproth.

METEOROLOGICAL JOURNAL,

For JANUARY, 1811,

Kept by ROBERT BANCKS, Mathematical Instrument Maker, in the STRAND, LONDON.

DEC. Day of	THERMOMETER. 9 A. M.	9 P. M.	Highest in the Day	Lowest in the Night.	BAROMETER, 9 A. M.	RAIN, noted at 9 A.M.	WEATHER. Day.	Night.
28	37°	35°	39°	29°	30·08		Snow	Fair
29	34	34·5	38.5	28	·36		Hail	Ditto
30	32	32	34	27	·50		Fair	Ditto
31	31·5	32	33	26	·50		Snow	Cloudy
JAN.								
1	30·5	32	32	26	·26		Ditto	Ditto
2	29	30	30	24	·04		Ditto	Snow
3	27	29	30	21·5	29·71		Ditto	Ditto
4	26	28·5	32	23	·90		Cloudy[b]	Cloudy[b]
5	25	28	30	23·5	·87		Ditto[c]	Ditto[c]
6	25	26	30	24	·80		Fair[c]	Fair[c]
7	27·5	29·5	29·5	24	·88		Ditto	Ditto
8	27	27·5	30	25	·84		Ditto	Ditto
9	29·5	31·5	32	23·5	·81		Snow	Cloudy
10	25	35	35	35	·93		Foggy	Rain*
11	41	37	42·5	34	·68	·130	Fair	Fair
12	40	44	45	34·5	·66		Rain	Rain
13	38	44	44·5	40	·65	·060	Ditto	Ditto
14	45	44·5	47	41	·63	·120	Ditto	Cloudy
15	45	37	45	32	·62	·015	Fair	Fair
16	36	38	40	37	·86		Ditto	Cloudy
17	46	47	50	34	·77	·045	Cloudy	Ditto
18	41	37	42	32	·77		Rain	Fair
19	35	38	41	32	30·32	·020	Ditto	Ditto[a]
20	35	34·5	36	31	·40		Fair	Ditto
21	35	39	40	32	·05		Rain*	Foggy
22	35	34	37	33	·21		Foggy	Ditto
23	37	40	41·5	34	·28		Ditto†	Ditto
24	37	38	41·5	33	·42	·010	Fair	Fair
25	36·5	35	37·5	28	·50		Ditto	Cloudy
26	31	41	42	37	·26		Ditto	Ditto
27	40	35	41	27	29·61	·030	Ditto	Ditto

·430 Inch. since last Journ. not including Snow.

[a] Intervening fogs. [b] Stormy. [c] Boisterous. * Immeasurable, † and small rain.

Erratum last Journal.—Dec. 25, for 29·31 read 29·00.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

MARCH, 1811.

ARTICLE I.

On the Motion of Rockets both in Nonresisting and Resisting Mediums. By W. MOORE, *Esq.*

(Continued from vol. XXVII., p. 285.)

To Mr. NICHOLSON.

SIR,

IN my last paper on Rockets I ascribed the invention of those now used by the English to Colonel Congreve:—My assertion rests in the fact that this gentleman has been rewarded by government for the discovery; and my own conviction, that, anterior to such reward being granted, a proper Invention of military rockets.

a proper enquiry could not but have been instituted concerning his pretensions.

I herewith send you a further continuation of my essay on the motion, &c. of these machines, which I hope will be found worthy your acceptance; and am,

Sir,

Your much obliged humble servant,

W. MOORE.

Royal Academy, Woolwich,
Jan. 1811.

PROP. II.

To determine the path of a rocket near the Earth's surface, neglecting the resistance of the atmosphere.

Path of a rocket in a nonresisting medium.

If during the time the rocket was on fire, the weight of the whole mass remained conftant, the path of the rocket would, by mechanics, be a straight line: but this being not the case on account of the continual wasting of the matter which feeds the flame of the rocket, the accelerative force of the body will be different at every instant; and therefore, since the accelerative force of gravity (as we will suppose) is constant to the height which rockets generally ascend, the rout of the rocket will consequently be a curvilinear one.

Now, although gravity acts to impel the rocket from a straight course, yet it does not hinder the same from arriving at

at a line parallel to the direction in which that force is exerted in the *same* time that it would have done by the single action of its own impelling force. Therefore, in order to determine the curve which the rocket will describe under the true circumstance of its motion, let us suppose that if gravity had not acted, the rocket would have arrived at C, Pl. V. fig. 1, in the line of its first direction A C in he time t. Then in this case we shall have (Prop. 1.) the space described in the time t, namely $AC = x = \frac{bc}{2am} \times$

$$\left(t^2 + \frac{ct^3}{3am} + \frac{c^2t^4}{6a^2m^2} + \frac{c^3t^5}{10a^3m^3} + \frac{c^4t^6}{15a^4m^4} + \&c.\right)$$

Or multiplying by $\frac{2am}{bc}$ (= suppose Q) and calling the coefficients of the several terms of the series A, B, C, &c.; it will be $Qx = t^2 + At^3 + Bt^4 + Ct^5 + Dt^6 +$ &c.; which reverted into a series of x is $t = \overline{Qx}^{\frac{1}{2}} - \frac{A}{2}.$

$$Qx + \frac{5A^2 - 4B}{8}.\overline{Qx}^{\frac{3}{2}} + \frac{3AB - 2A^3 - C}{2} Q^2x^2$$

$$+ \&c. = Q^{\frac{1}{2}} \times \left(x^{\frac{1}{2}} - \frac{A}{2}.Q^{\frac{1}{2}}x + \frac{5A^2 - 4B}{8}.Qx^{\frac{3}{2}} + \frac{3AB - 2A^3 - C}{2}.Q^{\frac{3}{2}}x^2 + \&c.\right);$$

the time of describing the distance x along A C from the commencement of motion.

Now C D (y) being the distance descended by gravity in the same time; we therefore get $\frac{1}{4}\sqrt{y}$ (omitting the $\frac{1}{12}$) for the time of the rocket's describing C D by the force of gravity: and consequently $\frac{1}{4}\sqrt{y} = Q^{\frac{1}{2}} \times \left(x^{\frac{1}{2}} - \frac{A}{2}.Q^{\frac{1}{2}}\right.$

$$\left. x + \frac{5A^2 - 4B}{8} Qx^{\frac{3}{2}} + \&c.\right)$$

Hence, knowing the equation which subsists between A C and C D the track which the rocket describes may be drawn; for it will only be necessary to give some value to x in order to determine the corresponding value of y, and to lay off this upon C D drawn perpendicular to A B,

 and

and thus finding several points of the curve, the curve itself may be described.

We have here supposed gravity to act in parallel lines, which is not strictly true; but the distance to which a rocket ranges on the earth's surface being so very small compared with its circumference, the errour arising from the contrary supposition will not in any material degree affect our conclusions.

Prop. III.

To find the velocity of the rocket in the curve at any given instant.

Velocity of a rocket at any given time.

In the preceding diagram let A C $= x$, and A D $= z$ being the space described by the rocket in the time t: Then calling the velocity at C ($= b \times$ hyp. log. $\frac{am}{am - ct}$ (Prop. 1.) V; the velocity at D, in the curve, will be expressed generally by $\frac{\dot{z}\,V}{\dot{x}}$, following from the laws for the resolution of motion. Now by the laws of falling bodies in vacuo C D $= g\,t^2$: and putting k and l for the natural sine and co-sine (to rad. 1.) of the angle C A B of projection; we shall have A B $= l\,x$, C B $= k\,x$, and D B (the ordinate of the curve) $= k\,x - g\,t^2$. Therefore $\dot{z} = \left((k\,\dot{x} - 2\,g\,t\,\dot{t})^2 + l^2\,\dot{x}^2\right)^{\frac{1}{2}}$ and v $(= \frac{\dot{z}\,V)}{\dot{x}} = \left(\frac{l^2\,\dot{x}^2 + (k\,\dot{x} - 2\,g\,t\,\dot{t})^2}{\dot{x}}\right)^{\frac{1}{2}}$ $\times$ V. Again by the theory of variable motions $\dot{x}$ $= V\,\dot{t}$. Consequently $v = \left(\frac{l^2\,V^2\,\dot{t}^2 + (k\,V\,\dot{t} - 2\,g\,t\,\dot{t})^2}{V\,\dot{t}}\right)^{\frac{1}{2}}$ $\times V = \left(V^2\,l^2 + (k\,V - 2\,g\,t^2)\right)^{\frac{1}{2}} = \left(l^2\,b^2 \times (\text{hyp. log.}\ \frac{am}{am - ct})^2 + (k\,b \times \text{hyp. log.}\ \frac{am}{am - ct} - 2\,g\,t^2)\right)^{\frac{1}{2}}$ the velocity of the rocket at D; which wants no correction: because when $v = o$, $t = o$, and the whole vanishes: therefore

therefore $v = \left(l^2 b^2 \times \left(\text{hyp. log. } \frac{a\,m}{a\,m - c\,t}\right)^2 + (k\,b \times \text{hyp. log. } \frac{a\,m}{a\,m - c\,t} - 2\,g\,t)^2.\right)^{\frac{1}{2}}$

When the angle of projection is $90°$, $l = 0$, and $k = 1$: therefore v in this case will be $b \times$ hyp. log. $\frac{a\,m}{a\,m - c\,t} - 2\,g\,t$; as determined in Prop. 1: and when $k = 0$, or the action of gravity is 0, the velocity of the rocket in its rectilinear path is $b \times$ hyp. log. $\frac{a\,m}{a\,m - c\,t}$, which agrees with what has already been observed.

Prop. IV.

To find the horizontal range of the rocket, having the angle of elevation of the engine, and the time the rocket is on fire given.

Horizontal range of a rocket.

Let D, fig. 2. be the place of the rocket when all the wild-fire it contains is just exhausted; and C m and C n the measures of the velocities of the rocket in the directions A C and D I, the latter of which is a tangent to the curve at D: then by trig. sin. $\angle$ C $n\,m$ ($= n$ C B $=$ I D B) $= \frac{C\,m}{C\,n} \times$ sin. $\angle$ C $m\,n = \frac{C\,m}{C\,n} \times$ co-sin. of the angle of elevation of the engine $= \frac{\text{Vel. at C}}{\text{Vel. at D}} \times$ co-sin. of the $\angle$ C A B. Whence calling the velocities at C and D, V and v (computed from the 3 Prop.) we have sin. $\angle$ I D B $= \frac{V}{v} \times$ co-sin. $\angle$ C A B. Since, then, we have found the $\angle$ I D B, it will be easy to determine that part of the range denoted by B L. For the curve from D being a parabola D H $= \frac{s\,u\,v^2}{g}$, and V E $= \frac{s^2\,v^2}{4\,g}$ (from the laws of projectiles in vacuo); where s and u represent the sin. and co-sin. of the $\angle$ I D H $= \angle$ I D B $- 90°$; consequently V F $=$ V E $+$ E F $=$ V E $+$ D B $= \frac{s^2\,v^2}{4\,g} + k\,x - g\,t^2$; whereof x is given by the first proposition.

Again,

Again, by the nature of the parabola V E : V F :: E H^2 : F L^2 $= \frac{u^2 v^2}{g} \times \left(\frac{s^2 v^2}{4g} + kx - gt^2\right)$; and F L $=$ $\frac{uv}{4} \times \left(\frac{s^2 v}{4g} + kx - gt^2.\right)^{\frac{1}{2}}$ Whence A L the entire range $(=$ F L $+$ B F $+$ A B$)$ $= \frac{uv}{4}\left(\frac{s^2 v^2}{4g} + kx - gt^2\right)^{\frac{1}{2}} + \frac{suv^2}{2g} + lx$ which was required.

For an example in numbers: suppose the engine from whence the rocket is thrown to make an angle with the horizon $= 45°$: and let all other things remain as in the first proposition. Then v, the velocity of the rocket in the curve at the end of its burning $= \left(l^2 b^2 \times (\text{hyp. log.}\ \frac{m}{m-c}\right)^2 \times \left(kb + \text{hyp. og.}\ \frac{m}{m-c} - 6g\right)^2\Big)^{\frac{1}{2}} = \sqrt{4479024 + 4080400} = \sqrt{8559424} = 2925{\cdot}6$; and sine angle I D B $= \frac{V}{v} \times$ co-sin. $\angle$ C A B $= \frac{2993}{2925{\cdot}6} \times$ co-sin. $\angle$ C A B $= 134° 6' 38''$. Whence angle I D H $= 44° 6' 38''$; the nat. sin. and co-sin. of which are ·6960172 and ·7180251 $= s$ and u respectively: and the values of the letters in the above expression for the range collectively are as under.

s	$=$	·696
u	$=$	·718
v	$=$	2925·6
k	$=$	·7071
l	$=$	·7071
x	$=$	4159·6
g	$=$	16
t	$=$	·3

Whence the range itself is readily found $=$ 273116·29 feet or 51·72657 miles.

On the Motion, &c. of Rockets in a resisting Medium.

PROP. V.

Motion of a rocket in a resisting medium.

The strength or first force of the gas from the inflamed composition of a rocket being given, as also the weight of the quantity of composition the rocket contains, together with the

the time of its burning, and the weight and dimensions of the rocket; to find the height to which it will rise in the atmosphere if thrown perpendicularly, and also the velocity acquired at the end of that time; the law of resistance being supposed to vary directly as the square of the velocity; and the laminæ of the composition to fire uniformly and to burn parallel to the rocket's base.

Put $w =$ weight of the case of the rocket and head
$c =$ weight of the whole quantity of matter with which it is filled
$a =$ time in which the same is consuming itself uniformly
$n = 230$ ozs.
$s = 1000$
$d =$ diameter of the rocket's base
$x =$ P D the space the rocket describes in the time t
$v =$ the acquired velocity in that time
R $=$ the resistance of the air to the rocket when moving with a velocity of b feet per second

Then $b^2 : v^2 :: \mathrm{R} : \frac{\mathrm{R}v^2}{b^2}$ the resistance at D; and consequently $sned^2 - (m - \frac{ct}{a}) - \frac{\mathrm{R}v^2}{b^2}$ (see Prop. 1) will be the motive force of the rocket at D in this case; and $\frac{(sned^2 b^2 - \mathrm{R}v^2)a}{(am - ct)b^2} - 1$ the accelerative force. Therefore $\dot{v} = 2gf\dot{t} = \frac{(sned^2 b^2 - \mathrm{R}v^2)\,2ga\dot{t}}{(am - ct)b^2} - 2g\dot{t}$; or putting $2ag \times sned\ b^2 = h$, $2ag\mathrm{R} = k$, $amb^2 = l$ and $cb^2 = p$, we shall have $\dot{v} = \frac{h\dot{t} - kv^2\dot{t}}{l - pt} - 2g\dot{t}$; and $l\dot{v} - pt\dot{v} = h\dot{t} - kv^2\dot{t} - 2gl\dot{t} + 2gpt\dot{t}$; and further, putting $h - 2gl = q$ to render the expression as simple as possible, it will be $l\dot{v} - pt\dot{v} - q\dot{t} + kv^2\dot{t} + 2gpt\dot{t} = o$; where v may be determined in terms of t as follows;

Assume $v = \mathrm{A}t + \mathrm{B}t^2 + \mathrm{C}t^3 + \mathrm{D}t^4 + \mathrm{E}t^5 +$ &c.; then making $\dot{t} = 1$; we have $\dot{v} = \mathrm{A} + 2\mathrm{B}t + 3\mathrm{C}t^2 +$

4 D

$4Dt^3 + 5Et^4 +$ &c.: and substituting these in the given equation it becomes as below; namely,

$$
\begin{aligned}
l\dot{v} &= lA + 2lBt + 3lCt^2 + 4lDt^3 + 5lEt^4 + \text{\&c.} \\
-pt\dot{v} &= -pAt - 2pBt^2 - 3pCt^3 - 4pDt^4 - \text{\&c.} \\
-q\dot{t} &= -q \text{ - - - - - - - - - - - - - - - -} \\
kv^2\dot{t} &= \qquad\qquad -kA^2t^2 - 2kABt^3 - (2AC + B^2) \\
&\qquad\qquad\qquad\qquad \times kt^4 - \text{\&c.} \\
2gp\dot{t} &= \quad 2gpt \text{ - - - - - - - - - - - - - - -}
\end{aligned}
$$

By adding which we evidently obtain

$$0 \quad = 0 \quad 0 \quad 0 \quad 0 \quad 0$$

Whence, equating the homologous terms, to find the quantities A, B, C, &c. they become

$$A = \frac{q}{l}$$

$$B = \frac{pq - 2gpl}{2l^2}$$

$$C = \frac{p^2q - 2gp^2l + kq^2}{3l^3}$$

$$D = \frac{p^3q - 2qp^3l + 2kpq^2 - 2gkpql}{4l^4}$$

$$E = \frac{35kp^2q^2 - 52gkp^2ql + 8k^2q^3 + 12g^2kl^2p^2 + 12p^4q - 24lp^4q}{5l^5}$$

&c. &c.

Therefore the fluent required is $v = \frac{q}{l}t + \frac{pq - 2glp}{2l^2}t^2 + \frac{p^2q - 2glp^2 + kq^2}{3l^3}t^3 + \frac{p^3q - 2lp^3q + 2kpq^2 - 2gklpq}{4l^4}t^4 + \frac{35kp^2q^2 - 52gkp^2ql + 8k^2q^3 + 12g^2kl^2p^2 + 12p^4q - 24lp^4q}{5l^5}t^5 +$ &c. And hence the space x is readily found $= \frac{q}{2l}t^2 + \frac{pq - 2glp}{6l^2}t^3 + \frac{p^2q - 2glp^2 + kq^2}{12l^3}t^4 + \frac{p^3q - 2lp^3q + 2kpq^2 - 2gklpq}{20l^4}t^5 +$ &c.; where, making $t = a$ in both expressions, the values of v and x as required by the proposition will be determined.

SCHOLIUM.

SCHOLIUM.

In this solution, the resistance of the air to the rocket's motion is supposed to vary directly as the square of the velocity; an hypothesis which experiments disprove when applied to military projectiles with cannon balls: but it is to be apprehended, that in the motion of rockets the deviation from this law is scarcely to be regarded, since, what takes place in the flight of shot and shells to violate it, is in a great measure obviated in the rockets, by the extreme heat of the flame that rushes from them; this rarifying the ambient air promotes the motion of the particles striking the head of the rocket towards its hinder parts; and since it is only the immediate motions of such particles backwards that can cause the law to obtain (for it would obtain correctly, if, after the impact of the particles they had no power to impel others lying before them, but either glided off from the surface struck, or had their force annihilated by it at the moment of striking) it is to be expected that the conclusions here brought out which are grounded on this law of resistance, will be found to agree pretty correctly with the results determined from experiment. Scholium.

But if they should not, let then the law of resistance be as the *n*th power of the velocity; and the method of solution will remain precisely the same as before. For it is only the fourth equation in the preceding process, namely, $k v^n t' =$ &c. that will vary or become affected by any deviation from the law we have assumed; and therefore when this shall have been settled by experiment (the only way in which it ever can be settled), and the absolute resistance determined in any one case of velocity, and the real strength of the rocket composition ascertained; then, and *not till then*, shall we be able to offer any *unerring* rules to the military practitioner.

II.

II.

The beautiful Tint of Flowers acquired by the same Means that paint the Rainbow. In a Letter from Mrs. AGNES IBBETSON.

To Mr. NICHOLSON.

SIR,

Dissection of the corolla of flowers.

AS I mean to dedicate the present year to the consideration of the interior of plants in general, I cannot better begin it, than by giving to the public the dissection of the *corolla of flowers*, a subject in itself so very curious, that I scarcely know any in botany (where utility is not concerned) that more merits the attention of the inquisitive. By taking the petal of each different sort of flower, and splitting it, drawing off the upper and under skins, and leaving only the pabulùm or middle part to be examined singly, and then magnifying each skin alternately, the most exact result is gained; which I shall now lay before you.

Petals of flowers derive their beauty from the aqueous particles in their pabulum.

The petals of flowers owe none of their beauty to the colour that paints them, which is, when drawn off, *dull* and *dead*: nor do they owe their brilliant tints to the *skin* that covers them: but the greatest part of their loveliness is derived from the *bubbles of water*, that compose their pabulum. Receiving the sun's rays they are enlivened and brightened by the reflection and refraction, from those *drops of water*, and from that spot of light seen in every bubble of water, and striking to the focus underneath them; by which means the whole flower would at times be a blaze of light, had not nature, to soften it, covered the petal with an upper and under skin, which curtails their diamondlike rays, and leaves them only a lightness and beauty unequalled. Uncovered they would resemble the rainbow formed by the same means (though coloured by the division of light): but, shaded as they are with a gauselike matter, they acquire a more chastened tint, and are equally delightful, without being oppressive to the sight. It must be remembered, that I treat only of flowers which have *regular* petals,

petals, and touch not on the cryptogamiæ, or any that possess not those direct parts.

Five kinds of corolla.

Taking the corolla of flowers in this strict sense, they may be divided into five different sorts: the thin petal, such as the *rosa*; the moist petal as found in the *hyacinthus*; the velvet denoted by the *viola tricolor*, and sweet scabious; the thick petal by the *magnolia* and *lilium*; and lastly, the everlasting, by the xeranthemum.

Petals differ from leaves in their colour being external.

The petals of most flowers differ from leaves in many respects, but particularly in one essential point; in leaves the coloured part is within, and the upper skin (even of the darkest leaves) is white; but in flowers, the coloured part is without, and the *white* is within; for almost every flower (the orange excepted) have their pabulum or interior white.

Thin petals. The rose.

Most thin petals (which I shall first describe), have, like the rose, when its upper surface is peeled off and examined, an extremely thin skin, in which are tiny bladders of pink liquid, woven in a sort of gauselike texture. But when this is taken off, it displays a pabulum of white or rather water bubbles, to which nature, (to lessen the brightness,) has added two circular white lines, which give a tenderness to the pink impossible to describe. The common violet is formed in the same manner; but the darker colour and thicker skin lesssen the sparkling of the water.

The common violet.

Experiment to show how much colours are brightened by the sun's rays transmitted through water.

To prove that bubbles of water are the cause of the beauty that flowers generally transmit, either in vivid flashes, or tender tints, to the retina; take the dullest colour that was ever painted, and, filling a small glass bubble with water, let the rays of the sun fall through it on the colour, and it will become the brightest and most beautiful imaginable; and exactly resemble the tint of flowers. It was this discovery, that made me more than five years ago dedicate a whole summer to dissecting the petals of flowers, which I suspected to be beautified and enlivened by water. I then proved my ideas were just, that their tints were formed by colours transmitted through water, and like clouds and the rainbow owed their perfection to this cause; but I shall recur to this subject at the conclusion of my letter.

The

Moist petal. The second or moist petal is so filled with water, that it only excites our astonishment how such a thin gauselike matter can contain such a quantity of liquor; and yet the flower reposes on the hand without wetting it. No person can examine it, and not see that it owes its sparkling appearance to the water it contains; for when we look at it in the sun, it almost dazzles the eyes. When I said that flowers owed the beauty of their tints to the bubbles, I did not mean all their charms: there are other arts by which they shine, and well deserve that encomium our blessed Lord bestowed: "not Solomon in all his glory was arrayed like one of these."

Velvet petal. The velvet petal I was long discovering the formation of. I saw it differed from every other, yet could not understand how, as it was evidently in the upper surface; for when this was taken off, its pabulum resembled that of the rose: but putting it a little sideways in the solar microscope, I soon found out the secret. Its structure. A vessel extremely diminutive is carried up and down in a scallop, as at *a a*, Pl. V, fig. 10, and each of the upper points divided. Why this produces such extreme softness, it would be very curious to inquire. The fact however is certain; it is by this means we make velvet, chenille, &c.; it is this that causes such extreme softness in feathers in general; in miniature painting it is this makes a stroke so much softer than a dot; and is it not the down on the cheek of beauty, which gives so exquisite a softness, that paint must directly destroy? Is is that the ray of light, instead of being repelled or absorbed, is reflected down the scallop, and therefore declines by degrees? It is curious that our method of making velvet should be precisely the same by which these flowers are formed, so that the studying of nature would have taught us that art. But how many secrets may she not teach, if she was pursued with avidity! To proceed, this scallop is found in every flower, which has this velvetty appearance, though in some it is deeper, in others less deep, and therefore not so soft: still it is always a scallop.

Thick petal. The next petal I am to explain is the thick. It is composed of a thick but double surface skin; the pabulum having fourteen or sixteen layers of extremely diminutive round

round bubbles, placed in rows. If the sun falls on the magnolia, when the surface is taken off, it will shine like so many little stars. Sometimes the upper skin is in round bladders, sometimes long; but the former produces the most brilliant appearance. These thick flowers always decay the soonest, as the little water they contain is quickly evaporated.

Everlasting petal.

The everlasting in its name declares its nature. Unlike all the rest, it retains a part of its beauty, and owes it to its being coloured by a *powder*, instead of *water*. I found little else in the bubbles, which are long, and have a very thin pabulum. But to show, that some of our flowers may owe their beauty to other contrivances beside water, I shall name a plant common in the fields, which greatly adorns them, and is known by the vulgar name of butterflower, the ranunculus. The petals appear to be varnished; but this arises from a powder, which exactly resembles calcined magnesia, and lies between the pabulum and upper skin. This is easily proved to be the cause of its shining appearance; for in the claw there is no white powder, nor is there any varnish. The powder in the everlasting flowers is probably formed in the same manner, and of the same nature, which will account for their not decaying: though the ranunculus does not last, because the pabulum under the white powder is full of water, which is not the case with the everlastings.

Manner in which the petal is supported & strengthened.

I shall next describe the means by which these thin transparent flimsy bodies are sustained and strengthened, so as to bear much pressure, and to preserve their elegant shapes often in spite of wind and rain. In describing the formation of leaves in a former letter, I mentioned two sorts of vessels belonging to them, the *nourishing* and *spiral vessels*: they equally belong to the corolla, and enter it up the claw of the petals, the latter forming always the middle stroke, while the nourishing vessels adorn each side: see Pl. VI, fig. 1. It is a beautiful provision of nature, that those muscles that serve to turn, to open and shut the flower, in short, to manage it in every way, should also be its means of strength; for the spiral vessels being in their cases very stiff and strong, when compared to a petal, certainly serve this

double

double purpose: and it is curious, that in some petals (the thick for example) where the thickness supplies the place of support, the spiral wires are often found without a case. It is in watching these little minute circumstances we come at the secrets of nature; for when we place her in unnatural situations, to understand her intentions, we only draw conclusions to deceive ourselves. No one would for a moment doubt, that these spiral wires governed the petals, who would watch them for hours as I have done for days together. I have seen a flower suddenly contract its muscles, even before the rest of the corolla could obey the motion; seen a sudden cold air draw them to breaking; or seen them expand with a hot sun, or on being breathed upon, till they could no longer obey any impulse. I know not any better means to show their muscles, than exposing flowers either to a very hot fire or an icehouse; both extremes act equally. The moment they contract with violence, so that each muscle is seen through its cover; then relaxing at once, they hang like mere wet rag, having neither form nor strength, and decay directly. On examining a dying flower, it will be found, that it is the muscles which draw the petal together, and not the skin. I know no better flowers for showing the power of the spiral wire, than the deadelphian tribe. The art with which the banner of all peaflowers is contrived has always excited my admiration. How admirable the management! and when, as in the visia faba, the case would not be strong enough to distend it, there is formed a double and treble layer of vessels, which form a strengthening piece for the whole, and which the case surrounds. Observe the strength of the keel, when the finger is passed over it; how admirably it is contrived to withstand both wind and rain, and to form a hollow in which the stamens and pistil are sheltered from every danger. Who can examine the wings of the lathyrus, and not be struck with astonishment at the art it betrays? how much thinking would it cost a mechanic, to invent such a contrivance! and how poor, how coarse, would his execution have been! Can any person see without admiration the petals of the iris susiana, and the elegance of those folds? But I should never have done if I expatiated on all the forms and shapes I have so often contemplated with delight, and dissected for the sake of admiring

Muscles of flowers.

miring the contrivance of the whole; it is only to be conceived when seen. But to my dear countrywomen in particular I would recommend such employment: I am most anxious to interest and excite to study those of my own sex, who are still ignorant how much pleasure is attached to the developement of the human mind. These contemplations are not puerile, as many may think; they speak to the heart.

Formation of the rainbow resembles the colouring of flowers.

Let me now turn to the rainbow, and endeavour to show how exact the resemblance is. It would be superfluous here to enter into a detail of the manner in which the rainbow is formed by the refraction and reflection of the solar rays by drops of water; and since we know this to be the case, what should prevent flowers, formed of little else from receiving part of their hues by this means, while the colour given them is increased and embellished by the water it is seen through, and the sparkling light, which passes into most petals and adds to their beauty? To try the effect I got a quantity of extremely small glass bubbles of water blown for me, and placed them as in a petal—in rows. Although infinitely larger, yet they appeared a petal extremely magnified. I then covered them with a gauze, painted to represent the flower, and truly did it imitate the sort of brightness and brilliancy it was intended to represent.

Jacobean lily.

In the jacobean lily the surface is formed of an extremely thin skin of red liquid, but the pabulum is composed of round bubbles of yellow water, which together appear orange; but what causes that fiery bright spark, which animates the whole? the prismatic scarlet, which proceeds from that point of light the circular ball always gives, and which sparkling, has puzzled so many botanists? This is easy to be seen when the upper surface is taken off; for it then really dazzles the eyes. What causes the orange tint in the lily of that colour, or the crown-imperial? Its first skin is red, and its next green. I appeal to any painter to say, whether these will make orange? and yet the flower is truly so. What causes that exquisite tint of green in the andromeda pulverulenta, for the upper skin and pabulum are all white? probably here it is reflection, which it often is, at the bottom of a flower, or the deepening of a bud.

Lily and crown-imperial.

The

The bud alters not its colour, but its situation is changed, and much will depend on this. How many times have I looked for a tint in a flower, and found nothing in the least to resemble it? I was once extremely fond of painting, and sought in flowers for colours their beauty tempted me to think I should find. But how mistaken I was! I accused my own awkwardness; but, after many trials, was assured, that the deception was in the flower, which owed little of its beauty to the liquid that inflated it, on the contrary I was then convinced I must seek for the cause elsewhere, for in no one instance did the liquid of the flower serve even to stain the paper of the same colour as the flower; and when used, some chemical aid, some process to brighten and preserve, must be resorted to; for it is always dull and poor, so that no one could guess its origin; a proof, that the flowers owed not their beauty to the liquid colour, but to something that aided and enlivened it. And drying flowers! what things are they when done? they are not only bad in themselves, but of the most fatal consequence to botany, where flowers are consulted; for they change their colours, get hairy, and undergo many more alterations, which deceive, and cause the most unpleasant mistakes.

Paints not to be obtained from the juices of flowers.

Changes in flowers when dried.

As to the scent in flowers, it is very easy to trace their secretions, being in bladders, and always divided from the other juices (as in leaves). They are generally fixed between the upper skin and pabulum; though in some few flowers (as in the geranium) they are to be found in the scallop of the petal. The scent is in every flower an oil, and must be extremely strong, when such diminutive bladders can so perfume the air; if we could gain it pure, it would not want any chemical aid to strengthen it. It was those bladders that used to be taken for the largest pores in flowers.

Smell of flowers from an oil contained in bladders,

which have been mistaken for large pores.

I have now given a short sketch of corollas in general, I shall finish therefore by a few observations on the pores observed in flowers; and endeavour to show the extreme difference in botany between the two terms perspiration and evaporation. If I had published this letter two years ago, which I was on the point of doing, I should have said, as all the botanists do, that there were two sorts of pores

Difference between perspiration and evaporation.

Two sorts of

pores in flowers. But I have now too deeply studied the part to be longer deceived. There are certainly none, either large or small, above or below the petal. Take the petal of a rose, and, drawing off the upper surface, divesting it of as much pabulum as possible, lay it on your finger, the under part uppermost; with a clean soft rag rub the part as long as you see any mark; and then place it on the glass of the microscope. If, on being magnified, it shows any black pieces, rub it again, and they will all disappear; and you will find its pinholes were merely the impression of the apertures in the pabulum, which showed through a skin of such extreme fineness, that it can be compared only to gold beaters skin; and that the surface of the petals is formed of this, and extremely thin pink coloured bladders, in which no pores are to be found, that are visible even with the solar microscope. Du Hamel thought he had found this sort of skin on the inside of the rind of trees and all plants: I before proved his conjecture true in this respect; which put an end to the possibility of branches and stems perspiring; but I had no idea I should find this skin in flowers. I have now in my sliders thirteen sorts of this skin, which keeps well, taken from such different orders and classes of flowers, that I must believe it is universal in all. Perhaps, but for this skin, these thin and flimsy flowers would never be able to bear any heat, and the water in them would directly evaporate. It is very astonishing to consider how it can be preserved in such delicate shapes. That there are airholes I do not in the least doubt, though they cannot be perceived; for we have proof, that vapour constantly escapes; but certainly no water, for a fluid requires a much larger aperture. Therefore in flowers perspiration and their imbibing the dew drops are both mistakes. I have no doubt the hairs carry water, and the dew, to the interior of the petal; but, that no water can pass through the skin, which I have just described, I have the most positive proof.

Pores supposed in flowers, but there are none.

Rose leaf covered with a thin skin destitute of pores,

found also on the inside of the rind of trees and plants,

and apparently on all flowers.

But it has airholes.

Flowers give out no oxigen, but yield a great deal of other air, of so bad a quality that no candle can burn in it, and which does not diminish nitrous acid. It is however extremely moist; and in the various trials I have made, to endeavour

Flowers yield no oxigen, but an azotic air containing much moisture.

endeavour to ascertain the quantity of moisture formed in this vapour, which plants constantly yield, I have always been foiled, from the quickness of the evaporation. In weighing leaves, I fold them in blotting paper, first ascertaining its weight: but though the leaves have often lost near one thirtieth of their weight in two hours, yet the paper has scarce gained one third of that thirtieth: which, considering the different weight of air and water, is nothing, and certainly less infinitely than the moisture in that vapour.

Plants yield vapour only.

That plants yield vapour only, and that the moisture is very difficult to be separated from the air, is easily proved, by the method in which it is generally received. Put a plant fresh out of the ground, first wiping of the watery cryptogamiæ that may grow on it, and place it under a bell glass, and in an hour or two it will cover the interior with dew: but leave an opening in any part not so big as a sixpence, and no moisture will be found. Why is this? because in the first place, the air being pressed by the quantity of oxigen that pours from the plant, is by this pressure forced to give out its moisture; and the fluid, being detached from the air, is attracted by the glass, to which it has great affinity. But when the oxigen vapour has the power to escape, not being pressed, it retains its moisture, and both leave the glass in the same condition in which they quitted the pores. So I found it in weighing the leaves, which I have tried in every manner possible: the blotting paper was not pressure sufficient to detach the moisture, and the paper absorbed but little, though the leaves had evidently lost so much vapour.

Plants do not perspire.

In former letters I endeavoured to show, that the watery cryptogamiæ and the vapour just mentioned were taken for the perspiration of plants: every trial, every year's study, convince me still more of this truth: *that plants do not perspire.* That they yield a quantity of vapour, I never doubted—but who, that has ever dissected or studied plants, can confound the terms of *perspiration* and *evaporation* together, in botany at least? Perspiration is the issuing of water from pores formed for the purpose. It was said by all the advocates of perspiration, to be accelerated by heat, retarded by cold, and ascertained by

by measurement: whereas evaporation is air and water mixed, issuing from pores so very diminutive, that no eye, even aided by the solar microscope, can see them. It can neither be accelerated, nor retarded; nor can it be ascertained, (since it disappears as fast as it is collected for the purpose). I before showed what the watery cryptogamiæ were, by drawings of many plants of the sort; nourished by the dews of the atmosphere; and I think it will be granted me, that perspiration could not have stalks, which most of these watery bubbles have. It is curious they should always turn milk-white.

Watery cryptogamiæ always turn milk white.

Flowers in hot climates (at least most of them) are thick in the petal, probably to guard the muscles from heat. To trace the effect of heat and cold in plants, to trace different climates in their productions, is the most delightful study; and one congenial to this, my present employment. Who can dissect the betula, and not be assured it is formed to endure the extremities of cold? Formed of 14 or 16 separate *rinds* (not barks) something like the skin I have before described, with a powder between each, how could frost approach it? Who can dissect the water lily with its large and prominent air vessels fluted, and with vacuities managed like the fish either to raise it in water or depress it, and not be sure it is formed to live in that element? Who can dissect the boletus, formed like a sponge, and guarded in its interior, without seeing it is to grow in caverns and under ground? or the *extreme* alpine plants, with their watery and open vessels, that they want little more than the dews of the atmosphere, and are not to depend on their rocky heights for food? But it is impossible long to dissect plants, and not see there are certain marks and indications, that give proof of all this.

Adaptation of plants to the places where they are to grow.

I am, Sir,

Your obliged servant,

AGNES IBBETSON.

Explanation of the Plates.

Plate V. figs. 3, 4.—Rose petal: fig. 3, upper skin, pink; fig. 4, pabulum, white.

 Figs. 5,

Figs. 5, 6.—Violet petal: fig. 5, upper skin, purple; fig. 6, pabulum, white.

Figs. 7, 8, 9.—Crown-imperial and orange lily; fig. 7, first skin red; next, fig. 8, the net, green; and underneath, fig. 9, the pabulum of a dirty yellow white; which together make the flower of a beautiful rich orange.

Figs. 10, 11.—Jacobean lily: fig. 10, the upper skin, red; fig. 11, pabulum, yellow bubbles, imparting its fiery tint from the divifion of light in them.

Fig. 12.—The heartsease, cut in thickness to show the rows at top and bottom of the petal.

Fig. 13.—The thick magnolia petal, cut in thickness, but not so much magnified.

Fig. 14.—Everlasting flower cut in thickness.

Plate VI, fig. 1.—The banner of the bean flower, *x x* spiral vessels.

Fig. 2.—A case holding the spiral vessels, with the nourishing vessels at the side.

Fig. 3.—Petal of the rose.

Fig. 4.—Wing of the lathyris flower.

Fig. 5.—Keel of a lathyris, *x* spiral vessels.

III.

Analysis of a Soil containing free Muriatic Acid, and Muriates of Soda, Lime, Alumine, Magnesia, and Manganese. By the Rev. WILLIAM GREGOR. *Communicated by the Author.*

Saline matter AS I was passing through a lane, in my neighbourhood, some months ago, I was struck with the appearance of some saline matter on the soil, which formed the base of a hedge. This soil was what agriculturists, I presume, would denominate "a compact yellow loam."—It was not "*made ground*"; for the road had been evidently cut through it.—The spot is situate in the middle of a hill, of gradual ascent, in the parish of Merther, in the county of Cornwall, about a quarter of a mile from the river, which flows through Tressilian bridge, which is a tide river. But, if

on a compact yellow loam.

Situation.

if we may judge from circumstances of situation, this soil is removed beyond the reach of any direct or assignable influence, which the river can have upon it. I collected a sufficient quantity of this soil with a view of examining it at my leisure.

It has a peculiar smell—distilled water, that had stood upon some of *it in the cold*, instantly reddened litmus paper. I extracted all that was soluble in this soil by distilled water. A portion of this solution was gradually evaporated in a retort: what came over into the receiver, examined from time to time, proved to be pure water, until the contents of the retort were reduced to a small compass, and began to assume a thick consistence: then the fluid, which distilled over, acquired the properties of an acid. And the modes of proof usually resorted to announced the acid to be the *muriatic*.

Treated with cold water, and part of the solution evaporated, yielded muriatic acid.

The common reagents, employed upon another portion of this solution, indicated the presence of the sulphuric and the muriatic acids, of lime and of some other earth or earths, and of the oxide of some metal. For liquid prussiate of potash, when dropped into it, caused an abundant greenifh white precipitate; and spirituous tincture of galls, after a time, produced a turbidness, and a greenish gray matter was deposited.

Tests indicated also sulphuric acid, lime and other earths, and an oxide.

The solution is, at first, colourless and transparent: if it be evaporated, it gradually contracts a brownish hue: and vegetable extractive matter is deposited on the sides of the vessel. Sulphate of lime is first separated. When this substance has ceased to be produced, beautiful crystals of muriate of soda make their appearance: when these, by farther evaporation, have been exhausted, the remaining fluid cannot be brought to produce any other crystallized salt. It has a brownish tint, its transparency is in some measure impaired, and it strongly reddens paper stained blue by litmus.

Vegetable extract, sulphate of lime, muriate of soda.

If some of the solution be evaporated nearly to dryness, alcohol digested upon the mass separates sulphate of lime and muriate of soda. What the alcohol has dissolved consists of *muriates*, and vegetable extractive matter, for the fluid is brownish. It strongly reddens litmus paper.

The solution evaporated, and alcohol added.

If

If this solution is mixed with water, and ammonia dropped into it, an abundant brownish matter is thrown down; the clear decanted fluid, which is no longer rendered turbid by ammonia, yields up a small quantity of lime to the *carbonate* of ammonia. The matter, which ammonia has precipitated, becomes dark brown by ignition. Various experiments prove, that it consists of the *oxide of manganese*, and the earths *alumina* and *magnesia*. The salts, then, which this soil contains, appear to be *sulphate of lime*, and the respective *muriates* of *soda*, *lime*, *alumina*, *magnesia*, and *manganese*. It contains, also, *muriatic acid* in a disengaged state, and vegetable extractive matter.

Oxide of manganese, alumine, and magnesia.

Contents of the soil.

Analysis to ascertain their proportions.

I wished to form some judgment as to the quantity and relative proportion of these salts in a given portion of the soil. With this view, I extracted all that was soluble by distilled water in sixteen ounces of soil previously freed as much as possble from stones. The solution was gradually evaporated, until no more sulphate of lime would appear. It was washed with alcohol mixed with water, collected and dried it = 13·75 grains. When, upon farther evaporation, the muriate of soda ceased to crystallize, the fluid was poured off, and the salt washed with alcohol. Upon evaporating the alcohol mixed with water, a small quantity of muriate of soda was obtained. The *whole quantity* of muriate of soda amounted to 117·25 grains. What was poured off from the last salt obtained was added to the residuary fluid: and ammonia was dropped into it, as long as it produced any effect. The browniſh precipitate was catched upon a filter. It required a much larger quantity of distilled water to edulcorate it sufficiently, than I could have expected. After exposure to a low red heat, it = 13·8 grains—carbonate of ammonia precipitated a small quantity of lime from the remaining fluid. This carbonate of lime was dissolved in muriatic acid; and in the state of dried muriate it scarcely amounted to two grains.

Muriatic acid.

The fluid, thus freed from earthy and metallic substances by the means of ammonia, was rendered slightly acid by pure nitric acid, and assayed by liquid nitrate of silver. The muriate of silver, which was precipitated, was sufficiently edulcorated. It was dried by being placed, during several

several hours, in a silver crucible upon heated iron. It = 142·8 grains, equivalent to 22·8 of muriatic acid.

The 13·8 grains precipitated by ammonia were boiled to dryness in a ley containing 20 grains of potash; and the matter remaining in the silver crucible, which had resisted the action of the potash, after exposure to a low red heat, = 8 grains.

What the potash had dissolved was separated by muriate of ammonia from its solvent. It was *alumina*, and must have amounted to 5·8 grains. The 8 grains of residuum above mentioned became, by ignition, of a dark brown colour. Sulphuric acid did not apparently act upon it, until a small piece of sugar had been added. The sulphuric solution was decomposed by perfectly saturated carbonate of ammonia added to excess. After the precipitate thereby produced had been separated by the filter, and sufficiently edulcorated, phosphate of soda separated from the clear fluid the triple salt of magnesia first noticed by Dr. Wollaston. It = 3·9 grains, which, according to the calculation contained in Dr. Marcet's very ingenious and scientific analysis of the Brighton waters, indicate 0·74 of magnesia, which reduces the remaining substance to 7·26 grains. It had all the properties of *manganese*. What proportion of acid the muriates of alumina and manganese respectively contain, has not, I believe, been accurately ascertained.

Alumine.

Manganese.

I should not have considered the facts above detailed as being worthy of record, if I had not coupled them with the recollection, that, several years ago, I had examined a soil, which I have reason to think contained *precisely the same saline substances* as the subject of this letter. *Muriate of alumina* is not, indeed, mentioned in my notes upon it, but the results of the experiments which I *then* made agree so exactly with what I have recently observed, that I have not the least doubt, that muriate of alumina was present also, with the other salts. I did not then detect it, as in *the then state* of chemical means, it was not so easily discoverable. That soil contained also a free acid, and the muriate of manganese, with the other salts, which I have before enumerated. It differed, in one respect, from that which I lately examined: in having a much larger quantity of vegetable extractive matter mixed with it. The spot where

A similar soil ob-erved several years ago.

seventeen miles distant from this.

where that soil was found is about a mile from the town of Helstone, and it is distant above seventeen miles, in a direct line, from the spot described in this letter. It is possible, that this sort of soil may frequently occur, although it has, I believe, been unnoticed—the presence of a free acid and the muriate of manganese communicate to it properties, by which it may be quickly and easily *recognized.*

WILLIAM GREGOR.

Creed, Jan. 16, 1811.

IV.

Observations on the Oxides of Iron, with a Discussion of their Nature; by Mr. J. H. Hassenfratz*.

An accurate knowledge of the oxides of iron of great importance in chemistry,

IRON is the most useful of all metals; and is that which is most generally met with in all the substances we know. It is found in them either in the metallic state, or combined with oxigen. Chemists separate it in the state of oxide in most of their analyses: sometimes indeed they obtain it in the metallic form, but in this case it always retains a portion of the carbon employed in its reduction. The proportion of oxigen combined with the iron before and after analysis, in the oxide previously existing, or in the oxide obtained, has long been the subject of a discussion, which has occasioned errours in the estimations of products, from the little agreement that has existed and still exists among chemists.

and chemists differ much on the subject.

Berthollet asserts, that oxigen may combine with a metal in any proportion, from the minimum to the maximum;

The celebrated author of the Chemical Statics is persuaded, " that the proportions of oxigen in metals may vary progressively, from the limit at which combination first becomes possible, to that at which it has attained its highest degree†". A chemist whose opinions are of great weight, Proust, endeavours to establish the principle, that metallic oxides, and that of iron in particular, have but

* Annales de Chim. vol. LXIX, p. 113. For a valuable paper on this subject by Dr. T. Thomson, see our last volume, p. 375.

† Statique chem. tom. II, p. 370.

two

two degrees of oxidation; one at a minimum, the other at a maximum*. Thenard, known for the accuracy of his experiments, asserts, that there are three sorts of oxides of iron; the white, the green, and the red†.

Proust, that there is no union but in these two proportions: Thenard, that the proportions are definite, but may be more than two.

According to Berthollet, "the oxidation of metals, and the properties of the oxides they form, depend on the strength of their affinity for oxigen, their power of cohesion, their fusibility, their volatility, the degree of oxidation they are capable of attaining in consequence of these qualities, the condensation the oxigen undergoes in them, and the quantity of caloric it contains."

Berthollet's doctrines.

Now as the affinity of oxigen for metals, as well as their cohesion, fusibility, and volatility, must vary in proportion to the quantities of this substance already combined with them, it follows, that "*the combination of oxigen* (in metals) *may vary in them, and even indefinitely*, from the point when, the force of cohesion losing its preponderance, oxidation becomes poſſible, to that point where it ceases to be so, unless the mutual affinity of the two elements be assisted by some other affinity, which extends the limit of oxidation farther."

Proust deduces his two degrees of oxidation from the colour of the precipitates obtained in solutions of iron on the addition of prussiates, alkalis, gallic acid, and hydrosulphurets.

Arguments of Proust.

Thoroughly saturated prussiate of potash precipitates from its solution iron at a minimum in the state of white precipitate, which afterward becomes green; and it precipitates iron at a maximum in the state of blue prussiate.

Alkalis precipitate oxidulated iron from its solutions in a green oxide, which gradually turns black: iron oxided at a maximum they precipitate red: and when a solution contains iron in both these states, the red is precipitated first, and the black afterward.

Gallic acid produces no change in solutions of oxide of iron at a minimum; that which is at a maximum it precipitates black.

* Ann. de Chim. vol. XXIII, p. 85.

† Ann. de Chim. vol. LVI, p. 57: or Journal, vol. XIV, p. 224.

Sulphuretted

Suphuretted hidrogen produces no effect on solutions of iron at a minimum; while in solutions where the iron is at a maximum of oxidation it is decomposed, reducing the iron to the state of oxidule.

Those of Thenard.

Thenard distinguishes these three kinds of oxides by the action of alkalis, of gallic acid, and of prussiates, on solutions of iron; particularly on the solutions in sulphuric acid.

From their solutions in sulphuric acid alkalis precipitate the oxide of iron at a minimum white, the oxide at a medium green, and the oxide at a maximum red. These three oxides may be separated in succession from a solution in which they are mixed.

The gallic acid precipitates nothing from solutions of iron oxided at a minimum; from solutions in which the iron is oxided at a medium it throws down a green gallate; from that in which it is at a maximum a black gallate.

Lastly, the prussiates are blue and green with iron oxided at a minimum; green and blueish with that which is oxided at a medium; and blueish and blue with that which is at a maximum.

Bucholz considered Thenard's white oxide as a subsulphate, which the latter controverts.

Mr. Bucholz having made some observations on the white oxide of Mr. Thenard*; and attempted to maintain, that it was nothing but a combination of black oxide and sulphuric acid; the learned French chemist has pointed out a process, in a note published in the Bulletin des Sciences of the Philomathic Society†, by which the white oxide of iron may be obtained pure, and freed from sulphuric acid.

The proportions of oxigen not given by Berthollet,

The learned author of the Chemical Statics mentions none of the proportions of oxigen in oxide of iron. Indeed according to his principles he could have pointed out only those of the two extreme terms, that at which the combination becomes possible, and that at which it attains the highest degree; but to make known these two proportions we must be precisely acquainted with these two extremes.

or Thenard.

Thenard contents himself with giving the characters of the

* Journal fuer die Chemie und Physick. Berlin. No. 12, p 721.

† Nouveau Bulletin, ann. 1808, No. 5, p. 96.

the three species of oxide he admits, without attempting to determine their proportion of oxigen.

The celebrated author of the System of Chemical Knowledge, Fourcroy, announces, that the proportions of oxigen, deduced from the experiments of the illustrious Lavoisier, are, for the oxidule, from 25 to 27 parts of oxigen to 100 of iron; and for the oxide, from 40 to 49*. Proportions assigned by Fourcroy,

Proust, who has defended with so much acumen two degrees of saturation of iron only, the one at a minimum, the other at a maximum†, carries the quantity of oxigen in the former as far as 28 to 100 of iron, in the latter to 48. Proust,

Mr. Bucholz, a German chemist of deserved reputation, who has lately attempted to determine the proportion of oxigen in the two degrees of oxidation‡, states the minimum proportion of oxigen at 30 to 100 of metal, and the maximum at 42. and Bucholz.

From this diversity of opinion on the degrees of oxidation, and the proportions of oxigen in the different oxides, between chemists equally celebrated for their skill and their precision in analysis, I conceived it might be of use to give a succinct account of the attempts that have been made to solve this question, which is of essential importance to chemists. This is the only motive, that has led me to publish the following abstract of the experiments that have been undertaken, to attain a knowledge of the oxide of iron, and determine its different degrees of oxidation. The diversity of opinions between them led to the following examination of what has been done on the subject.

Of the methods of oxiding or disoxiding iron, by means of which the proportions of oxigen, that combine with this metal, may be determined, the six following are the principal that have been employed: 1st, oxidation by the action of air and heat: 2d, the reduction of oxides of iron, either by the action of heat alone, by that of heat and carbon, Principal methods employed for oxiding iron.

* Syst. de Conn. chim. tom. VI, p. 160 and foll. English transl. p. 214 and fol.

† Ann. die Chim. tom. XXIII, p. 85. Journal de Phys. 1804, tom. II, p. 33.

‡ Jour. fuer die Chem. and Phys. Berlin, No. 12, p. 696. and fol.: Journ des Mines, No. 131, p. 361 and fol.: or our Journal, vol. XXV, p. 353.

carbon, or by that of heat and hidrogen: 3d, oxidation by other metallic oxides: 4th, by water; 5th, by acids; and, 6th, by nitre. I shall examine the results obtained by each of these methods separately.

1st Method. *Oxidation by air and fire.*

Simple calcination by Schæffer, Schæffer appears to be the first chemist, who observed, that iron increased in weight by exposure to the air and heat*. He remarked, that this increase, after a complete calcination, was one third the weight of the iron, consequently 33·3 to 100 of metal.

Guyton, Guyton repeated Schæffer's experiment†. He found, that iron filings calcined in a Hessian crucible, under the muffle of an open furnace, increased $\frac{1}{1}\frac{4}{7}\frac{3}{6}$, or 27·4 to 100 of iron. Steel filings, burned in the same manner, increased 38·58 per cent.

Lavoisier, Lavoisier, calcining iron filings in the same way, obtained four oxides, which had increased respectively 30, 32, 32·41, and 33 on the 100 of iron‡.

Darso, Mr. Darso calcined iron repeatedly, taking it out, and triturating it in a mortar, after each time of exposing it to the fire. By these means he obtained oxides, which increased successively 20, 30, 36, 40, 45, 50, and even 56 on 100 of iron filings§.

Hassenfratz, I repeated with much care the experiment of Mr. Darso, but could never obtain more than 42 on 100 of iron filings without stirring them while calcining, and 45 with stirring them‖: which leads me to suspect some errour in Mr. Darso's results.

Bucholz, Bucholz calcined iron filings in a crucible, exposing them to a red heat, and stirring constantly. The iron acquired an increase of 24 to 100 of the metal.

and Lavoisier again. Lavoisier, repeating the experiment of Ingenhousz, who burned fine iron wire in oxigen gas, burned iron turnings

* Swedish Transactions, 1757.

† Digression académique, Essai sur le Phlogistique, p. 8.

‡ Mém. de l'Ac. des Sciences, 1782, p. 525 and fol.

§ Journ. de Phys. 1806, tom. II, p. 294: or Journal, vol. XVII, p. 221, 267, 328.

‖ Ann. de Chim. vol. LXVII, p. 309: or Journal, vol. XXVI, p. 147.

in

in a vessel under a jar filled with oxigen gas, and standing over mercury. He found 100 parts of iron increased to 135 or 136 in one experiment, and to 132·414 in another*.

This difference of results in similar experiments shows the great difficulty of executing them properly: and among all that have been mentioned mine is the only one, in which the iron appears to have been carried to its maximum of calcination; as that of Mr. Darso, though indicating a larger proportion of oxigen, I conceive to have been inaccurate. In my experiments iron oxided at a maximum appears capable of being carried as far as 45 of oxigen to 100 of iron, or about 31·1 of oxigen to 68·9 of iron, which may be called in round numbers 31 per cent of oxigen. Remarks on these experiments.

As to the other experiments, which carry the oxidation to 24, 27, 30, 33, and even 38, we may presume, that portions of metal not yet completely calcined still remained among the iron, for on triturating the iron, after each calcination it was observed, that *little nodules of iron remained a long time, which the oxigen had not attacked.*

2d Method. *Disoxigenation of the oxide by heat alone, and by heat and carbon or hidrogen.*

Attempts have frequently been made, to carry back the oxide of iron from its maximum by calcining it strongly, either alone, or with substances that have a stronger affinity than it has for oxigen. Attempts to abstract oxigen from the oxides, by heat alone,

Mr. Berthollet asserts, that, on calcining red oxide of iron in porcelain tubes, it gave out no portion of oxigen when exposed to a powerful heat, though its colour appeared to change from red to black†. by Berthollet,

Fourcroy too asserts, that on calcining oxide of iron no oxigen was evolved; but he pretty constantly obtained carbonic acid, when the oxide assumed a black colour‡. Fourcroy,

Mr. Bucholz, in the paper already quoted, observes, that red oxide of iron being exposed in Hessian crucibles to the action of a powerful fire, the weight of the crucibles and oxide was diminished a few grains. This loss, if it Bucholz,

* Traité élém: de Chim. tom. I, p. 41.

† Journ. de Phys. 1805, vol. II, p. 562.

‡ Mém. et Obs. de Chim. p. 102, 103.

it can be ascribed to oxigen disengaged from the iron, would be about 6 per cent.

and Fourcroy again.

Fourcroy, calcining 144 grains of oxide of iron in a porcelain crucible hermetically closed, found that the apparatus had lost 50·6 grains, though the lute was fused, and the lid united to the crucible by an earthen glass.

Remarks on these experiments.

These different results in experiments, that may be considered as similar, leave some doubts on the conclusions, that Bucholz draws from his. In fact, the crucibles he employed were vitrified; which shows, that they had been exposed to a higher temperature than that in which they were made. This increased temperature of the crucibles therefore leaves the cause of the loss of weight, which the crucible and oxide had undergone, a matter of doubt. It may as well be ascribed to water remaining in the earth of the crucible, as to oxigen disengaged from the iron.

Attempts with substances containing carbon, by Bucholz;

Bucholz also endeavoured to diminish the oxigen in the oxide of iron by triturating and heating it with substances containing carbon; for instance, carbonate of ammonia, supertartrite of potash, wax, lampblack, and succinate of ammonia. With the first three the oxide experienced only an imperceptible diminution and reduction; and with the others the diminutions were very variable. With the lampblack one portion of the oxide had undergone no change, while another was reduced to the metallic state.

The oxide and oxidule of iron may be completely disoxidated and reduced by heating or fusing them with substances containing carbon and hidrogen.

with charcoal, by Hassenfratz;

At Moustier we fused with charcoal a variety of oxidulated iron in dodecaedral crystals from a mine in the valley of Aost. This ore, which contained 0·98 of oxidule and 0·02 of silex, yielded a button weighing 0·76. If no silex had been present it would have weighed 0·775. Supposing the metal to have retained 0·005 of carbon, it would follow, that this oxidule contained 30 of oxigen to 130 of iron. Two oxidules from the isle of Elba, one assayed at the laboratory of the Practical School of Mining, the other at the laboratory of the Council of Mines, registered No. 263, yielded 0·72 of iron. The first contained 0·05 of

foreign

foreign matter: and if we may suppose the second to have contained the same quantity, and each of the buttons to have retained 0·01 of carbon, it would follow, that these oxidules were composed of 33 oxigen to 100 of iron.

Priestley and Chaussier both disoxidated iron by raising it to a high temperature, and thus exposing it to the action of hidrogen gas. Amédée Berthollet has since announced the same result in a paper read to the Institute for 1807. As neither of these gentlemen has pointed out the diminution of weight of the oxidule in these experiments, Mr. Charbaut, a pupil at the Practical School at Moustier, and myself repeated them in the summer of 1807. We found, that 11·58 grains of oxidule of iron from the island of Elba lost in reduction 3·34 grains; whence it would follow, that 140·53 of oxidule contained 40·53 of oxigen to 100 of iron; supposing it to be perfectly pure, which is very probable; though of this we are not certain, as we did not analyse it.

and with hidrogen gas, by Priestley, Chaussier, A. Berthollet,

Charbaut, and Haffenfratz.

From these experiments we may conclude, that it is very difficult, if not impossible, to carry back the oxide, either by means of heat alone, or of heat and carbon, to the state of oxidule. From the reduction of the oxidule of the valley of Aost by charcoal it would appear to consist, taking the nearest round numbers, of 77 iron and 23 oxigen.

Remarks on these experiments.

At the conclusion of these observations we will examine, to what the difference between the oxidules of Aost and Elba is ascribable.

3d Method. *Oxidation of iron by metallic oxides.*

Iron has not yet been oxided by the help of any other metallic oxides, that have less affinity than itself for oxigen, but those of arsenic and mercury; because these two metals, when disoxidated, evaporate at a high temperature.

Oxidation of iron by oxides of other metals,

Guyton fused in a closed crucible equal parts of iron and oxide of arsenic. The latter metal evaporated, and the iron was oxided. The increase of weight was 27·1 on 100 of iron.

by Guyton,

Lavoisier fused iron both with oxide of arsenic and oxide of

and Lavoisier.

* Dig. Acad. Diss. sur le Phlogistique, p. 189.

of mercury. He found, that in these two fusions the iron increased from 35 to 40 per cent. In the table which follows his paper he gives the increase of iron by arsenic at 30 of oxigen to 100 of metal; and in his Elementary Treatise of Chemistry he quotes an experiment, in which iron treated with oxide of mercury gained 32 per cent.

No accurate inferences to be drawn from these.

It is not easy to deduce from these experiments any positive conclusion respecting the proportion of oxigen in the two degrees of oxidation, because, 1st, the quantities of oxigen combined vary too much: 2dly, the state of the oxide after oxidation was not accurately ascertained: 3dly, it is not certain, that the arsenic, which has a great affinity for iron, did not carry away some of it when volatilized. It appears too, that Guyton, in his experiment, did not mix a sufficient quantity of oxide of arsenic with the iron; for, according to the experiments of Proust*, the white oxide of this metal contain but 33 of oxigen to 100 of arsenic; whence the 100 parts of oxidecould only have yielded about 25 of oxigen to the iron.

4th Method. *Oxidation of iron by water.*

Oxidation of iron by water. Martial ethiops.

The black oxide has long been prepared in the shops, under the name of martial ethiops, by putting iron filings into a bottle full of water, shaking the mixture from time to time, and opening the bottle after every shaking, to let out the hidrogen gas evolved. To accelerate the process, Rouelle proposed to acidulate the water with vinegar, Croharé with a little nitric acid†.

Lavoisier.

Lavoisier found, that iron filings gained an increase of 30 or 35 per cent by agitation in pure water; and the same if the water were acidulated‡.

Cavazzely.

Cavazzely, on oxiding iron by means of water, obtained an oxide increased 35 per cent on the iron employed§.

Bucholz.

Bucholz, repeating the experiment of oxidation by water alone at the common temperature of the atmosphere,

* Journ. de Phys. 1799, tom. II, p. 150.

† Ann. de Chim. vol. XXXI, p. 333 and fol.

‡ Ac. des Sciences, 1782, p. 543 and fol.

§ Ann. de Chim. vol. XLIII, p. 94.

remarked,

remarked, that the oxide was covered with a crust of yellow oxide, while another part was in the state of black oxide.

The great variation in the weight of the oxide obtained by this method, and the observation of yellow oxide formed on its surface, show, that nothing can be deduced from experiments of the kind. These experiments of no account.

Lefebvre, Stoutz, and myself, when in Carinthia, satisfied ourselves, that red hot iron immersed in water decomposed it; the iron being oxided, and hidrogen evolved*. The illustrious Lavoisier, taking the hint from this experiment, heated little bits of iron twisted spirally in a copper tube, and passed the vapour of water through them while red hot. The water was decomposed, and the iron oxided. The iron had increased in weight 31·38 per cent. The hidrogen gas, which he collected and weighed, was found to be in the proper proportion for forming water with the oxigen combined†. Water assisted by heat. Lavoisier.

Bucholz, repeating the experiment of Lavoisier by passing the vapour of water through iron filings heated red hot in a close vessel, obtained a black oxide containing 76·7 of iron and 23·3 of oxigen. Bucholz.

The result obtained by Lavoisier appears accurate, and that of Bucholz rather too low. It is to be regretted that this process was not oftener repeated, but I shall notice this result again, when recapitulating those that may be considered as accurate.

(To be concluded in our next.)

V.

Some Remarks on the Observations and Experiments of Mr. Murray on the Nature of Oximuriatic Acid, and its Relations to Muriatic Acid. By Mr. JOHN DAVY.

To Mr. NICHOLSON.

SIR,

IT is generally admitted as a fundamental principle of modern chemistry, that all bodies not yet decompounded are Substances not decomposed to

* Mém. de l'Acad. des Sciences, 1781, p. 475.

† Traité élém. de Chim. tom. I, p. 92 and fol.

be deemed simple.

Difference between the phlogistic and antiphlogistic doctrine.

are to be considered as simple substances. In this respect the phlogistic principally differs from the antiphlogistic doctrine: the former is hypothetical, the latter theoretical. The phlogistic hypothesis in its most perfect form, as advanced by Mr. Cavendish, could not be confuted: it was simple, luminous, and perfectly well adapted to explain all the phenomena of chemistry; but it was still an hypothesis.

Difference between theory and hypothesis.

Theory is a history of generalisation and facts, hypothesis is a series of suppositions. Mr. Murray does not observe this distinction, he makes promiscuous use of the two words. Sometimes he speaks of the theory, and at other times of the hypothesis of Mr. Davy. I think it necessary to point out what to me appeared a loose mode of expression. It is of main importance, that expressions of facts be not misrepresented. It is of great consequence, that things be not termed notions—that theory be not considered as speculation.

Mr Davy's conclusions not hypothetical.

Before I examine the objections of Mr. Murray to Mr. Davy's conclusions, I shall endeavour to show, that these conclusions are not tainted by the slightest admixture of hypothesis. With this object in view I beg leave to recapitulate a few leading circumstances.

Formation of muriatic acid gas from oximuriatic and hidrogen.

Muriatic acid gas is formed by detonating together equal volumes of oximuriatic gas and hidrogen gas; and, if the experiment is correctly made, there is no condensation attending their union*.

Metals form peculiar compounds with oximuriatic gas:

The metals combine immediately with oximuriatic gas, and form with it a peculiar class of compounds, many of which are decomposed by water; and when this occurs, a metallic oxide and muriatic acid gas are always produced.

and the same compounds with muriatic acid gas, hidrogen being set free.

When the metals of the fixed alkalis, or the common metals, mercury, iron, tin, and zinc, are heated in muriatic acid gas, hidrogen gas equal to one half of the volume of the acid gas consumed appears free, and substances are produced exactly similar to those formed by the direct union of the same metals with oximuriatic acid.

The quantity of hidrogen not at all af-

In these experiments there is no difference in the proportion of hidrogen gas liberated

* I have seen this experiment made a number of times, in vessels that had been exhausted, and over mercury, and with similar results.

berated: it is not less, when the acid gas has been previously exposed to the action of salts having a strong attraction for water: nor is it greater in relation to the quantity of muriate formed, when liquid muriatic acid is used.

fected by the presence of water.

When metallic oxides are acted on by oximuriatic gas, oxigen gas is evolved; and when acted on by muriatic acid gas, water is produced. In the one instance the oxigen disengaged is the exact quantity contained in the oxide; and in the other case, the water produced has as much hidrogen, as existed in the muriatic acid gas, and as much oxigen as the metallic oxide contained.

Oximuriatic gas seizes the metal of an oxide, and expels its oxigen; which, if muriatic acid gas be employed, combines with its hidrogen, and forms water.

Such I conceive are the principal facts, which constitute the foundation of my brother's theory, that oximuriatic gas is a simple body. He combines this gas with hidrogen gas and forms muriatic acid gas. In his theory muriatic acid gas is a compound of oximuriatic gas and hidrogen. He combines oximuriatic gas with sulphur, phosphorus, and the metals: and in his theory, the resulting substances are compounds of the inflammable and metallic bodies respectively, and oximuriatic gas. Here we perceive no supposition, but a simple expression of facts, and this I humbly conceive is pure and genuine theory.

Mr. Davy's theory.

Having stated my opinion respecting Mr. Davy's views, I shall proceed to examine Mr. Murray's. This gentleman is of the old opinion, that oximuriatic gas is a compound body, consisting of muriatic acid and oxigen; that the muriates, substances formed by the combustion of the metals in oximuriatic gas, are composed of muriatic acid and metallic oxides: and likewise, that the constituents of the compounds resulting from the combustion of sulphur and phosphorus in oximuriatic gas are the respective acids of these two bodies and muriatic acid.

Mr Murray's, or the old theory.

Let it be kept in remembrance, that Mr. Murray does not mean, when he speaks of muriatic acid, the ponderable part of muriatic gas, which combined with ammonia, constitutes muriate of ammonia. The idea he attaches to the word is very different, for he says, that water is absolutely necessary to the existence of muriatic acid in the gaseous state; that muriatic acid has not yet been decomposed; and that it has an important and an anomalous relation to wa-

Mr. Murray's muriatic acid not the whole ponderable part of muriatic acid gas,

 ter,

ter, the investigation of which presents an interesting subject for inquiry.

but something contained in it,

What, it may be naturally asked, does Mr. Murray understand by muriatic acid? as he has given no definition of the word, I cannot say positively in what sense he uses it. This is certain, it does not signify muriatic gas, but, I should conceive, *that* part of muriatic gas, which combines with metallic oxides, and with the phosphoric and sulphuric acids. But the question now is, what is this peculiar substance? Has Mr. Murray examined it in its insulated state? has he described its properties? or has any chemist ever obtained it? The answer I may venture to assert is in the negative. To speak plainly my sentiments, muriatic acid gas, on Mr. Murray's hypothesis, appears to me to be a compound of water and an unknown basis, and the muriates compounds of this unknown basis and metallic oxides. And *this*, if I am not greatly mistaken in my opinion, is speculation in the strictest sense of the word.

which has never been obtained separate,

and is therefore hypothetical,

It is evident, that, whilst the basis is unknown, it is merely an imaginary substance. If this hypothesis should be preferred to Mr. Davy's theory, there should be for the sake of consistency a revolution of chemical doctrine. Phlogiston should triumph, and recover all it's former dignities. The arguments advanced in support of the simple nature of muriatic acid, might be used in *defence* of the compound nature of the metals. It has been already observed, that Mr. Cavendish's hypothesis of this kind is fully adequate to explain all chemical phenomena. This illustrious philosopher considered the metals and inflammable substances as probably compounded of unknown bases and phlogiston (hidrogen), and the metallic oxides and acids as probably compounds of unknown bases and water*.

and on a par with phlogiston.

Mr Cavendish's hypothesis.

Examination of Mr. Murray's objections to Mr. Davy's theory.

I shall no longer delay examining the objections made by Mr. Murray to Mr. Davy's theory, that oximuriatic gas is a simple body, having relations similar to those of oxigen to the metals and inflammable substances. Mr. Murray remarks, "that there is some improbability in the hypo-

Improbability

* Phil. Trans. for 1784.

thesis

thesis of two acidifying principles distinct from each other, but exerting similar chemical agencies," p. 139. It is generally admitted, that the only legitimate mode of reasoning in the experimental sciences and in chemistry in particular, is that of induction. We are not to judge of Mr. Davy's views, I humbly conceive, by considering their probability or improbability a priori, but by a reference to facts. I beg leave to take this liberty. Oximuriatic acid is attracted like oxigen by the positive and repelled by the negative pole of the Voltaic battery. Combined with hidrogen oximuratic acid constitutes muriatic acid. Combined with sulphur, phosphorus, and some of the metals, it forms peculiar compounds, possessing the property of neutralizing ammonia*. It may be objected, that these compounds do not change vegetable colours from blue to red. Granted. Neither does prussic acid. But this change is affected by sulphuretted hidrogen, which there is every reason to believe contains no oxigen, independent of that small portion which probably exists in sulphur. The action of oximuriatic acid on vegetable colours, and indeed all the chemical relations of this substance, are anomalous on the old hypothesis; for the sulphuric acid in its most concentrated state very slightly reddens dry litmus paper: and I find, that acetic acid does not redden it at all, when freed from water by long digestion with fused muriate of lime. Are not all these facts sufficiently decisive in their bearings to authorise the conclusion, that oximuriatic acid is an acidifying principle, similar to oxigen?

of two acidifying principles of no weight

Analogies between oxigen and oximuriatic acid.

That the compounds of the latter do not redden vegetable blues no objection.

To avoid all verbal controversy, I candidly acknowledge, that I consider the acidifying principle as a mere arbitrary term. Oxigen strictly speaking can with no more propriety be said to be the cause of the acidity of sulphuric acid, than sulphur. But if the term is to be still continued in use, we can only apply it to oxigen and oximuriatic acid, as these two substances are the only ones known to enter into the composition of a great variety of bodies having analogous acid properties.

Properly no acidifying principle.

* The only alkali on which they can act without double decomposition.

Mr.

Want of action between charcoal and oximuriatic gas.

Mr. Murray is of opinion, that the want of action between charcoal and oximuriatic gas is an anomaly in Mr. Davy's theory, which does not exist in the old hypothesis. To account for this fact he has recourse to a predisposing affinity, and to the supposition, that water is necessary to the existence of the muriatic and carbonic acids in a gaseous state. And he remarks, that, as water was not present when the experiment was made on the ignition of charcoal by Voltaic electricity in oximuriatic gas, it followed, that no alteration could be produced, that no oximuriatic gas could be decomposed, or any muriatic or carbonic gas formed. This reasoning would have been less unobjectionable, were it less hypothetical. Mr. Murray does not prove, that the muriatic and carbonic gasses contain water; he supposes that they do. The only fact advanced in favour of this constitution regards carbonic acid gas. It is well known, that the native carbonate of barytes differs from the artificial in being indecomposable by heat alone; and that this difference ceases, when the action of fire is assisted by steam or aqueous vapour. Mr. Murray supposes, that steam produces the decomposition by entering into union with the carbonic acid, and forming carbonic acid gas. I should conceive, that he would not have adopted this supposition, were he acquainted with Mr. Berthollet's observations on barytes. From this gentleman's experiments it appears, that the water does not combine with the acid, but with the earth, which it converts into a fusible hidrate, and thus liberates the carbonic acid, which immediately escapes in the gaseous state*.

Water supposed to be necessary to carbonic acid gas from its assisting the decomposition of native carbonate of barytes;

but this it does by converting the earth into a fusible hidrate.

Argument of Mr. Murray for the existence of water in muriatic acid gas

Though Mr. Murray has not endeavoured to demonstrate the presence of water in muriatic acid gas, he has attempted to show, that this probably is the case. When the acid gas has been exposed to the action of substances strongly attracting water, Mr. Murray observes, "it can only be affirmed, that a portion of water is abstracted from it, not that the whole of it has been withdrawn. As the water is removed from the acid gas, the affinity by which the remaining quantity is retained must prodigiously increase

* Memoires d' Arcueil, tom. II, p, 17.

crease in strength, until it equals or exceeds that of the substance attracting it; when of course its abstraction must cease," p. 135. If this statement be correct, it necessarily follows, that muriatic acid may exist as a gas in combination with different proportions of water. On the supposition that it does contain water, experience proves the contrary—for the proportion of hidrogen gas produced from muriatic acid gas acted on by different metals is always the same, whether it has been exposed to the influence of drying salts or not. I readily admit, that muriatic acid gas has important relations to water, and a strong attraction for this fluid; so too has fluoboracic acid gas: but I cannot allow the same relations and attraction to the muriatic acid of Mr. Murray—an imaginary body, that I confess I am altogether ignorant of. Moreover the strong affinity of muriatic acid gas for water is no more in favour of water being one of the constituents of this gas, than the strong attraction of potassium and sodium for oxigen is for the existence of oxigen in these metals.

proves that it may contain different proportions: but metals in all cases evolve from it the same quantity of hidrogen: Certain gasses have important relations to water.

When Mr. Davy concludes, that oximuriatic gas has relations similar to oxigen, I do not understand, that the expression implies an exact similarity between the attractions of these two substances, but merely a general analogy, such as the metals, or the inflammables have to each other, which allows of their being classed together.

Oxigen and oximuriatic gas analogous, but not in all respects similar.

If I may be permitted to give my opinion, I should say, we have no right to expect from a theory the explanation of ultimate facts. And the fact in question (the want of action between charcoal and oximuriatic gas,) is one of this kind. It is one of those, which constitute as it were the axioms of the science. For my own part I am glad, that it is not tortured by hypothetical explanation.

Explanation of ultimate facts not to be expected from a theory.

Having considered the reasons adduced by Mr. Murray for dissenting from Mr. Davy's theory, I shall proceed to examine the results of the particular experiments, which this gentleman considers more favourable to the old hypothesis than to the new views.

Mr. Murray's experiments examined.

Mr. Murray is of opinion, that the experiments of Mr. Cruickshank on the analysis of carburetted hidrogen gasses by oximuriatic gas are hostile to Mr. Davy's theory: as when

Conversion of oximuriatic and carburetted hidrogen

gasses into carbonic and muriatic acid,

not always the fact.

when the proper proportions of the two gasses were used, there was found to be a complete conversion of the whole into carbonic and muriatic acid gas. In answer to this objection it is only necessary to observe, that I have carefully repeated some of these experiments, making the detonations over recently boiled mercury, and never obtained carbonic acid gas though oximuriatic gas in great excess was employed. No regard was paid to the influence of water by the original author, therefore nothing can be inferred from the experiments of Cruickshank, but what is perfectly agreeable to Mr. Davy's theory. The presence of water is neglected by Mr. Murray: to him the only cause of ambiguity in the result appears to arise from the small portion of oxigen, which he supposes these gasses contain, and from which carbonic acid gas must be formed, when the hidrogen is separated by oximuriatic gas.

Action of oximuriatic gas on carbonic oxide

To avoid this source of ambiguity he has had recourse to carbonic oxide. He has repeated the experiment of Mr. Cruickshank on the action of oximuriatic gas on this compound, and has confirmed his results. He has very satisfactorily proved, that no change takes place in the two gasses when water is entirely excluded; and that, without the agency of water, no carbonic or muriatic acid gas is formed. The conclusion he draws from this fact is in favour of the old hypothesis. He takes it for granted, that "water is not decomposed by oximuriatic gas;" oxigen therefore he observes can only be afforded to the carbonic oxide to convert it into carbonic acid by the decomposition of the oximuriatic gas. It appears to me, that, even on Mr. Murray's own statement, his conclusion is gratuitous.

an insufficient proof,

since it may be otherwise accounted for.

It is not sufficient for his purpose to prove, that some carbonic acid is formed, but that the whole of the carbonic oxide is converted into this acid. For there is no improbability in the supposition, that some carbonic acid might be produced by the formation of a triple compound of oximuriatic acid, hidrogen, and carbon. The oil resulting from the union of oximuriatic gas and olefiant gas is a substance of this kind; and we know of several analogous cases in chemistry. Independent of what has been just said, have we not the most indubitable evidences of the decomposition of water

water by oximuriatic gas? Pass this gas and steam together through a tube heated to redness, oxigen gas will be produced, and muriatic acid gas formed. Detonate this gas and hidrogen gas together, and a similar formation of muriatic gas will take place. With these facts in view, who can hesitate in asserting, that water is decomposed by oximuriatic gas, that its hidrogen is attracted by this substance, and its oxigen consequently set free? Water decomposed by oximuriatic gas.

Since charcoal has no action on oximuriatic gas, is there any anomaly in the want of action between this gas and carbonic oxide? and since oximuriatic gas alone decomposes water, is there any thing unexpected in this effect being more readily produced by the joint attractions of oximuriatic gas and carbonic oxide?

Mr. Murray, to obviate every objection that might be raised to the formation of carbonic acid gas when water was present, made mixtures of carbonic oxide, hidrogen, and oximuriatic gas, over mercury, which were exposed to the solar light, or detonated by an electric spark. He used water for absorbing the muriatic gas formed, and constantly found great part of the carbonic oxide converted into carbonic acid. Carbonic acid produced from carbonic oxide, hidrogen, and oximuriatic gas over mercury.

I have assisted Mr. Davy in repeating these experiments; our mode of operating differed principally in this respect, we used ammoniacal gas instead of water for removing the muriatic gas, and we have obtained, probably in consequence of this, very different results. I beg leave to transcribe the account of one of the experiments, given by Mr. Davy in a note at the end of his Bakerian lecture for 1810. Different results obtained by Mr. Davy.

"We found that 7 parts of hidrogen 8 parts of gaseous oxide of carbon, and 20 parts of oximuriatic gas, exploded by the electric spark, diminished to about 30 measures, and calomel was formed on the sides of the tube. On adding dry ammonia in excess, and exposing the remainder to water, a gas remained, which equalled more than 9 measures, and which was gaseous oxide of carbon, with no more impurity than might be expected in the air in the gasses and the nitrogen expelled from the ammonia." In another experiment conducted in the same way as the last, a mixture of 22 measures of oximuriatic gas, of 8 hidrogen An experiment of his. Another experiment.

gen gas, and of 10 carbonic oxide, was inflamed by an electric spark over recently boiled mercury. There was a diminution produced equal to 6 measures, and some calomel formed. After the addition of ammoniacal gas, and the subsequent addition of water, there remained rather more than 13 measures of unabsorbable air, which burnt with the same coloured flame as carbonic oxide. We have made this experiment several times, and always obtained similar results*.

Difference of the results accounted for.

It appears therefore evident, that the formation of carbonic acid in Mr. Murray's experiments must either have been owing to his having used water, or to the gasses employed containing oxigen. Indeed he states, that there was a residue of common air in one of the experiments, which he considers of the most decisive nature. Now it is well known, that carbonic acid gas is formed, when carbonic oxide and atmospheric air are detonated together. We are not at a loss therefore to account for the difference of the results of Mr. Murray's experiments, and of those which we obtained.

In favour of Mr. Davy's theory.

The former were in opposition to Mr. Davy's theory, and on repetition they are not found to oppose it; and thus, instead of invalidating Mr. Davy's conclusions, they have quite a contrary tendency; they prove, that his theory will bear close examination, and that the facts, which at first

* The carbonic oxide used was procured from native carbonate of barytes and iron filings, and the oximuriatic gas from hyperoximuriate of potash by strong muriatic acid; the first portions only were preserved, which appear to be nearly pure oximuriatic gas.

New discovered compound of oximuriatic gas and oxigen.

Since this paper was written, Mr. Davy has discovered that there exists a very curious gaseous compound of oximuriatic gas and oxigen, which may be formed from hyperoximuriate of potash by diluted muriatic acid, assisted by a low heat: Possibly Mr. Murray's gas may have contained this substance, which we have found will convert carbonic oxide into carbonic acid. When *pure* oximuriatic gas is used, the results are as above stated.

From the particulars relating to this compound of oximuriatic gas and oxigen, described by Mr. Davy in his paper on the subject just read to the Royal Society, there are such additional proofs of oximuriatic gas being an undecompounded body, as must, I conceive, carry conviction to those who still adhere to the old hypothesis.

appear

appear inimical to it, do, when minutely investigated, afford additional proof of its accuracy, and of the incorrectness of the old hypothesis.

Mr. Murray was confirmed in the belief of the existence of oxigen in oximuriatic gas from the results of some experiment which he made with this substance and sulphuretted hidrogen gas. He states, that when a small quantity of the latter is admitted into a globe containing a large quantity of the former, no deposition of sulphur takes place; and only a slight cloudiness is produced, which soon disappears, and leaves the gas as transparent as at first. It is Mr. Murray's opinion, that sulphuric acid, or probably sulphurous acid gas, is formed, besides the muriatic acid gas. I shall silently pass by the experiments of this gentleman in which water interfered, for obvious reasons. And before I comment on the above results, I ask permission to mention those, which were obtained by Mr. Davy and myself in repeating this experiment in different forms. On adding 3 measures of dry sulphuretted hidrogen to 6 measures of oximuriatic gas in a jar over recently boiled mercury, there was a vivid inflammation, a condensation equal to 2 measures, and a slight deposition of sulphur. The colour of the gas was not entirely destroyed, but it soon disappeared, and the sulphur at first precipitated was no longer visible; but its place was supplied by a fine amber coloured dew, similar to the oximuriate of sulphur of Dr. Thomson, and there was a farther condensation equal to 2 measures. The muriatic gas thrown into the air had not the slightest smell of sulphurous acid gas, but it had the peculiar one of oximuriate of sulphur; and the water, by which some of it had been absorbed, produced only a slight cloudiness with muriate of barytes.

Mr. Murray's experiments with oximuriatic and sulphuretted hidrogen gas.

These repeated in different forms.

From these results it appears, that muriatic acid gas and oximuriate of sulphur are the only substances formed.

Results.

We have ascertained by direct trial that oximuriate of sulphur rises in vapour in muriatic acid gas. For the acid gas, after confinement over mercury in contact with this substance, had its very peculiar smell, and affected muriate of barytes with a cloudiness.

Oximuriate of sulphur rises in vapour in muriatic acid gas.

From Mr. Murray's account it appeared probable, that a gaseous

Gaseous triple

compound probably formed.

a gaseous triple compound of oximuriatic acid, hidrogen, and sulphur, was formed in his experiments, as he takes no notice of the appearance of any condensed fluid on the sides of his vessels. To ascertain whether this was the case, we have operated upon large quantities. Three cubic inches of sulphuretted hidrogen, allowed to pass from a receiver with suitable stop-cocks into a tube containing 6 cubic inches of oximuriatic gas, inflamed as it entered; a very slight cloudiness was produced, which soon disappeared; and there was an immediate formation of oximuriate of sulphur, which condensed on the interior of the tube, and was not redissolved by the muriatic acid gas, though heat was applied.

Experiment to ascertain this.

Precipitations of muriate of barytes accounted for.

In respect to Mr. Murray's results I have only to observe, that as he absorbed the muriatic acid gas by water in the very vessel in which the experiment was made, the solution, thus formed, should, as it indeed did, occasion a copious precipitate with muriate of barytes. It is now a well known fact, that the sulphuric and muriatic acids are formed by the decomposition of oximuriate of sulphur by water.

Habits of oximuriatic with sulphurous acid gas.

It may not perhaps be impertinent to the subject to state, that the relation of sulphurous acid gas to oximuriatic gas is precisely analogous to that of the gaseous oxide of carbon. I ascertained some months ago, that the two gasses previously dried admitted into an exhausted glass globe remain together without any alteration taking place; but that if a little water be present, muriatic acid gas and sulphuric acid are quickly formed.

The old hypothesis not sufficient, to explain all the phenomena.

I have examined with that freedom, which the subject required, Mr. Murray's objections to Mr. Davy's theory, and his ingenious defence of the old hypothesis. An hypothesis, I humbly imagine, inadequate, if admitted, to explain all the phenomena, and inconsistent with many well authenticated facts. To illustrate this assertion, let it be granted, that Mr. Murray, avoiding all sources of inaccuracy, obtained carbonic acid gas by detonating his mixture of oximuriatic gas, hidrogen gas, and carbonic oxide. Mr. Murray allows, that muriatic acid is incapable of existing in a gaseous state without combined water: and I have stated facts, which appear to me

me, to demonstrate, that, if muriatic acid gas does contain water, the proportion is not variable, but uniformly the same; that the smallest quantity cannot be abstracted without destroying the gas, and cannot be added without condensing it and forming liquid muriatic acid. In the given instance the hidrogen combines with the oximuriatic gas, forming common muriatic gas, a compound according to hypothesis of muriatic acid and water; there is no superfluous water produced, only the exact proportion necessary to render the acid gaseous: it is impossible, therefore, that the other part of the muriatic acid, which surrenders its oxigen to the gaseous oxide of carbon to form carbonic acid gas, can become gaseous; water is necessary for this change, and no water, but that confined in the muriatic acid gas already formed, is present. Yet it does become, according to the experiment, muriatic acid gas, which is a contradiction, and by itself amounts to a proof of the inaccuracy of the hypothesis.

Mr. Dalton adheres to the old system,

Mr. Dalton, in the second part of his "System of Chemical Philosophy," adheres to the old opinion of oximuriatic gas being a compound of muriatic gas and oxigen. In this respect he agrees with Mr. Murray; but his ideas concerning muriatic acid gas are very different from those of the latter gentleman. According to Mr. Dalton, muriatic acid gas itself is a simple body, containing no water. To account for the hidrogen produced when potassium and sodium are heated in this gas, he supposes, that these metals are compounds, that they consist of the fixed alkalis united respectively to hidrogen, which hidrogen is liberated, when muriate of potash and soda are made in the circumstances just mentioned. I cannot pretend to say, whether, when he adopted this hypothesis, he was aware of its consequences; whether he was acquainted with the fact, that there is no singularity attending the action of the metals of the alkalis on muriatic acid gas, and that tin, zinc, iron, and mercury, produce a similar separation of hidrogen from it. The necessary consequence of this view, if followed fairly, to its full extent, is the adoption of the doctrine of phlogiston. It is not my intention to discuss the merits of Mr. Dalton's speculations. I mention

but his ideas of muriatic acid gas differ from Mr. Murray's.

His supposition

leads to the revival of the

phlogistic doctrine.

I mention them merely as they offer a contrast to Mr. Murray's. The instances of these two gentlemen show the nature of hypothetical reasoning, and its tendency, when followed, to draw the ablest philosophers into difficulties.

I am, Sir, with great respect,

Your very humble servant,

JOHN DAVY.

London, Feb. 9, 1811.

VI.

An Estimate of the Height, Velocity, and Magnitude of the Meteor, that exploded over Weston, in Connecticut, December the 14*th,* 1807*: with Methods of calculating Observations made on such Bodies. By* NATHANIEL BOWDITCH, *A. M. A. A. S. and Member of the Philosophical Society held at Philadelphia.*

(Concluded from p. 97.*)*

Observations at Wenham, Weston, and Rutland.

Account of the appearance of the meteor at Wenham.

SOME time after the appearance of the meteor, I went with Mr. Pickering to Mrs. Gardner's house in Wenham, where she had observed the phenomenon. She informed us, that on the morning of the fourteenth of December, 1807, when she rose, she went toward the window of her chamber, which looks to the westward, for the purpose of observing the weather, according to her invariable practice for many years past. The sky was clear, except a few thin clouds in the west. It was past day-break, and by estimation about half an hour before sunrise, or seven o'clock. The meteor was immediately observed just over the southern part of the barn in her farm yard, nearly in front of the window; its disc was well defined, and it resembled the moon so much, that, unprepared as Mrs. Gardner's mind was for a phenomenon of that nature, she was not at first aware, that it was not the moon, till she perceived it in motion, when her first reflection (to use her own words) was—where is the moon going to? The reflection however was hardly made, when she corrected herself, and with her eye followed the body with the closest attention throughout its whole course. It

It moved in a direction nearly parallel to the horizon, and disappeared behind a cloud to the northward of the house of Samuel Blanchard, Esq. The true azimuth of the south part of the barn from the place of observation is N. 107° 59′ W. its altitude 3° 25′. The top part of the building is horizontal. The azimuth of Mr. Blanchard's house is N. 148° 22′ W. These buildings were useful in determining nearly the limits of the azimuths of the meteor.

Azimuths of the points of comparison.

The azimuths were obtained by observing with an excellent theodolite the difference of the azimuths of the sun and object, and finding the sun's azimuth by his observed altitude. The same method was made use of in obtaining the azimuths of the meteor. The angular elevation of the meteor above the horizon appeared always greater than that of the barn 3° 25′, and less than that of a tree in front of the window, along the branches of which the meteor ranged; the altitude of the top of this tree was 7° 10′. These fixed objects served to determine the altitude of the meteor, which is the most important element in the calculation, to a considerable degree of accuracy. After the theodolite was carefully adjusted, Mrs. Gardner directed the telescope attached to the instrument towards that part of the heavens where she first saw the meteor; the true azimuth was N 106° 54′ 54″ W. Altitude 5° 50′ 40″. The azimuth of a second place where it was seen was N 117° 35′ 54″ W. Altitude as before. The azimuth of a third place was N 132° 15′ 54″ W. Altitude 5° 29′ 40″. The azimuth of the place of disappearance was N 144° 33′ 54″ W. Altitude 4° 1′ 40″*. All these azimuths fall within the limits mentioned above, but the two last are undoubtedly too great. For a great circle passing through Wenham and Weston is inclined to the meridian of Wenham by an angle equal to 125° 18′ 38″, and as the two last azimuths exceed that quantity, they fall to the southward of Weston, which cannot be correct, because the meteor disappeared before it arrived at the zenith of Weston, as was observed by Judge Wheeler. It happens fortunately in the present instance,

Method of obtaining them.

Elevation and azimuths of the meteor from these observations.

* In the following calculations the allowances made for refraction in the above observations, were 8′ 50″, 8′ 50″, 9′ and 10′ 10″, making the altitudes respectively 5° 41′ 50″, 5° 41′ 50″, 5° 20′ 40″, and 3° 51′ 30″.

instance, that the other places of observation at Weston and Rutland are so situate with respect to Wenham, that a considerable errour in the azimuths at Wenham would not materially affect the result of the calculation made for determining the height or direction of the meteor, as will appear in the following calculations. Mrs. Gardner supposed the meteor to have been visible about half a minute.

How long visible.

In its progress it was occasionally obscured by thin broken clouds, which intercepted the view of it several times. No

Velocity.

train of light was observed to accompany it. Its velocity did not appear to be so great as that of shooting stars. Its colour was more vivid than that of the moon. The place of observation at Wenham is in the latitude of 42° 40′ 15″ N, and in the longitude of 70° 50′ 15″ W from Greenwich.

Appearance of the meteor at Weston.

By the observations of Judge Wheeler at Weston, published in the interesting memoir of Professors Silliman and Kingsley, in the sixth volume of the Transactions of the American Philosophical Society held at Philadelphia, it appears, that on the fourteenth of December, 1807, at about 6h. 30′, A. M., "numerous spots of unclouded sky were "visible, and along the northern part of the horizon a space "of ten or fifteen degrees was perfectly clear. The atten- "tion of Judge Wheeler was first drawn by a sudden flash "of light, which illuminated every object. Looking up "he discovered in the north a globe of fire, just then pass- "ing behind the cloud, which obscured though it did not "entirely hide the meteor. In this situation its appear- "ance was distinct, and well defined, like that of the sun "seen through a mist. It rose from the north, and pro- "ceeded in a direction nearly perpendicular to the hori- "zon, but inclining, by a very small angle, to the west, "and deviating a little from the plane of a great circle, "but in pretty large curves, sometimes on one side of the "plane, and sometimes on the other, but never making an "angle with it of more than four or five degrees. Its ap- "parent diameter was about one half or two thirds the ap- "parent diameter of the full moon. Its progress was not "so rapid as that of common meteors and shooting stars. "When it passed behind the thinner clouds, it appeared

"brighter

"brighter than before; and when it passed the spots of "clear sky, it flashed with a vivid light, yet not so intense "as the lightning of a thunder-storm. Where it was not "too much obscured by thick clouds, a waving conical "train of paler light was seen to attend it, in length about "10 or 12 diameters of the body. In the clear sky a brisk "scintillation was observed about the body of the meteor, "like that of a burning fire-brand carried against the "wind. It disappeared about 15 degrees short of the ze-"nith, and about the same number of degrees west of the "meridian. It did not vanish instantaneously, but grew, "pretty rapidly, fainter and fainter, as a red-hot cannon "ball would do, if cooling in the dark, only with much "more rapidity.—The whole period between its first ap-"pearance and total extinction was estimated at about 30 "seconds. About 30 or 40 seconds after this, three loud "and distinct reports, like those of a four-pounder near "at hand, were heard. Then followed a rapid succession "of reports less loud—so as to produce a continued "rumbling. This noise continued about as long as the "body was in rising, and died away apparently in the di-"rection from which the meteor came."—Mr. Staples observed, "that when the meteor disappeared, there were "apparently three successive efforts or leaps of the fire "ball, which grew more dim at every throe, and disap-"peared with the last. From the various accounts which "we have received of the appearance of the body at differ-"ent places, we are inclined to believe, that the time be-"tween the disappearance and report, as estimated by "Judge Wheeler, is too little, and that a minute is the "least time that could have intervened." The latitude of Weston is about 41° 15′ N, longitude 73° 27′ W from Greenwich, but there is a little uncertainty in both these quantities, though not enough to affect materially the result of the calculation.

Appearance of the meteor at Rutland.

The observations made at Rutland were procured by the kind offices of Professor Hall, of Middlebury College, Vermont, to whom Mr. Page communicated his valuable observations in a paper, expressed in the following terms. "I was at the west door of my house on Monday morn-"ing,

"ing, the fourteenth of December, 1807, about day-light; "and perceiving the sky suddenly illuminated, I raised my "eyes, and beheld a meteor of a circular form, in the "southwesterly part of the heavens, rapidly descending to "the south, leaving behind it a vivid sparkling train of "light. The atmosphere near the south part of the horizon "was very hazy, but the passage of the meteor behind the "clouds was visible, until it descended below the moun-"tains, about twenty miles south of this place. There were "white fleecy clouds scattered about the sky, but none so "dense as to obscure the tract of the meteor. I now la-"ment that I did not make more particular observations at "the time, and I should probably until this day have con-"sidered it to be what is commonly called a '*falling star*,' "had I not read in the New York papers an account of the "explosion of a meteor, and the falling of some meteoric "stones near New Haven, Connecticut, which, by recur-"ing to circumstances, then fresh in my recollection, I "found to be on the same morning that I observed the "meteor at Rutland. I am indebted to my learned friend, "Dr. Samuel Williams, for his aid and directions in ascer-"taining the situation of the meteor, when I first observed "it, and its course, and also for the order of my observa-"tions. *Form*, circular. *Magnitude*, less than a quarter "of the diameter of the moon. *Colour*, red vivid light, "*Tail*, or *train of light*, about eight times the length of its "diameter at the least, projected opposite to its course. "*Azimuth* when first observed, about 9° 30′ west of the "meridian. *Altitude* when first observed, about 18° 30′ *. "Descent to the south part of the horizon, west of the me-"ridian, by estimation 7 or 8 degrees. *Motion*, very rapid, "probably thirty seconds in sight. *Place of observation*, "Rutland, county of Rutland, and state of Vermont, la-"titude 43° 36′ N, as ascertained by Dr. Williams; longi-"tude west from Greenwich [72° 58′ 15″] as ascertained by "Mr. Bowditch, by calculations made upon observation "of the solar eclipse of June 16, 1806." In a letter, which accompanied the preceding paper, Mr. Page ob-

Description and place of it.

* In making use of this altitude, 3′ for refraction was subtracted, making it 18° 27′.

served,

served, that his recollection of the meteor was pretty distinct, and that he was enabled to determine its situation by the position of certain known objects.

Deductions from the preceding observations.

The observations made at Wenham, combined in various ways with those at Rutland and Weston, by the methods given in the preceding problems, furnish the results contained in Table 1, in which the given quantities are marked with an asterisk. These quantities are varied a few degrees in the different examples, for the purpose of forming an estimate of the change in the calculated place of the meteor, from any supposed errour in the observations. In the two first examples are combined (by Problem 2) the azimuth and altitude observed at the first appearance of the meteor at Rutland, with various supposed altitudes at Wenham. The 3d and 4th examples are like the first and second, except in a small change in the altitude and azimuth at Rutland. In the fifth and sixth examples, the first azimuth and altitude observed at Wenham are combined (by Problem 1) with the azimuth at Weston, supposing it in the 5th example to be north; and in the 6th N 3° W. The 7th and 8th examples contain the results from combining the same Wenham observation with the azimuth at Rutland, supposing it in the 7th example to be N 170° 30′ W, and in the 8th N 173° W. From the 9th to the 12th examples, the same calculations are repeated with the second Wenham azimuth and altitude. In the 13th example, the azimuth and altitude at Weston, at the time of the disappearance of the meteor, are combined with the altitude at Wenham, supposing it 5°: in the three following examples the calculation is repeated with small variations in the values of the given quantities. The azimuths at Wenham, calculated in the 13th and 14th examples, are made use of in the 17th, 18th, and 19th, with the corresponding assumed altitudes at Wenham, and the azimuth at Rutland at the time of the disappearance of the meteor, supposing it to be N 172° W in the 17th and 18th examples, and N 170° 30′ W in the 19th.

Calculations from the preceding observations.

It appears by this table, that in the 5th and 6th examples the

Calculations from the preceding observations.

altitude of the meteor at Weston was about 16° 30′, and in the 9th and 10th about 36°. Now if the meteor appeared at Weston to describe a great circle, beginning at the north part of the horizon and terminating at a point having the azimuth N 15° W and altitude 75° (which was nearly the case by the observations of Judge Wheeler) the azimuth corresponding to the altitudes 16° 30′ and 36°, would be respectively 1° 11′ and 2° 53′, as is easily found by spherics. The places of the meteor corresponding nearly to the azimuths 1° 11′ and 2° 53′, are given in the three right hand columns of the table opposite to those examples; these quantities being found by proportion. Thus by the 5th and 6th examples, the latitudes corresponding to the azimuths 0° and 3° are 42° 3′ 6″ and 42° 2′ 15″, varying 51″ by a change of 3° of azimuth; hence the latitude corresponding to the azimuth 1° 11′ is 42° 2′ 46″, or 42° 2′ 45″, as in the table. The other quantities were calculated in the same manner. The azimuth of the meteor at Wenham at the time of disappearing was not far from 123° 30′ or 124°, as is evident from examples 13, 14, 15, and 16. Mrs. Gardner supposed the meteor to have been visible considerably south of this point, which could not be correct for the reasons stated in the former part of this paper. By taking the second and third azimuths and altitudes of the meteor, as estimated by Mrs. Gardner, and finding by proportion the altitude corresponding to the azimuth 123° 30′ or 124°, and allowing 9′ or 10′ for refraction, the altitude will be found to differ but few minutes from 5° 30′, which is made use of in the 14th example. The estimated altitude would have been found nearly equal the same, if the four observations at Wenham had been taken into the calculation, by the usual method of interpolation, explained by Sir I. Newton in the Princip. Lib. 3, Lem 5*. The results of the 14th example are assumed in the right hand colums of Table I, and in Table II, as the true values at that time; and it is eiden t, that the latitudes and longitudes thus found must be nearly correct, since the meteor disappeared almost in the zenith of Weston, and a considerable errour in any of the observations

* In putting A for the azimuth at Wenham, and a for the corresponding altitude corrected for refraction, expressed in degrees and decimals, the formula of Newton gives $a = 5{\cdot}6972° - 0{\cdot}0009488° (A - 106{\cdot}90°)(A - 117{\cdot}60°) - 0{\cdot}00007012° (A - 106{\cdot}91°)(A - 117{\cdot}60°)(A - 132{\cdot}26°)$.

observations would not materially affect the result, as appears by comparing examples 13, 14, 15, and 16. This latitude and longitude agrees nearly with that obtained in the 19th example, by combining the azimuth and altitude at Wenham with the azimuth 170° 30′ at Rutland; from which it appears probable, that the azimuth of the meteor at Rutland, at the time of its disappearance, must have been about 170° 30′, and the corresponding *calculated* altititude at that place 5° 45′*; but, by Mr. Page's observations, the azimuth of the meteor at the time of its disappearance was 172° or 173°, being about 2° greater than by the preceding calculations; and it seems reasonable to make the same allowance on the first azimuth, observed at Rutland. This correction being made, the azimuths become 168° 30′ and 170° 30′, and the altiudes 18° 27′ and 5° 45′, corresponding respectively to the first and last observations at Rutland. The azimuth correponding to any intermediate altitude may be found sufficiently near by supposing the variations of altitude and azimuth to be proportional to each other. In this way the azimuths corresponding to the altitudes of 6° 30′ and 7° 30′† would be respectively 170° 23′ and 170° 14′; which are rather less than the azimuths made use of in examplesss 11 and 7. The changes to be made in the results of these examples, for this small difference of azimuth, may be easily estimated by comparing those examples with the 12th and 8th. In this way were found the assumed values, corresponding to those examples, given in the right hand columns of Table I. By taking the mean of the assumed values in Table I, corresponding to the examples 5, 6; and 7, 8; also the mean of those deduced from examples 9, 10; and 11, 12; there will be obtained the altitudes, latitudes, and longitudes of the meteor, marked in Table II, as the most probable values correponding to the times of the first and second observations made at Wenham.

Calculations from the preceding observations.

* Mr. Page states, that he saw the meteor till it descended below the mountains; but as it was hazy in the direction of the meteor, and the time early in the morning, it must have been difficult to determine by *observation* the precise point of its disappearance. The above method of *calculating* the altitude must give it very nearly corect.

† These are nearly the altitudes at Rutland at the time of the first two observations at Wenham, as appears by Examples 7, 8; 11, 12. An errour of several minutes in these altitudes would hardly cause any sensible change in the calculated azimuths at Rutland.

At

Calculations from the preceding observations.

At the first Rutland observation the meteor was not seen at Wenham, since the azimuth at Wenham at that time was probably less than 84° (as appears by the first four examples of Table I) and the first observed azimuth exceeded 106°; so that there can be no very accurate estimate of the situation of the meteor at that time. However, as the altitude of the meteor observed at Wenham, and its calculated altitude above the level of the sea (found in Table II), did not vary much during the time of its appearance at Wenham, it is highly probable that no change was experienced from the time of the first Rutland, to the first Wenham, observation. The preceding method of interpolation gives for the azimuth 83° 14′ 59″ at Wenham, a corresponding altitude at that place of 7° 43′*. This altitude with the azimuth at Rutland 168° 30′, and the corresponding altitude 18° 27′, give the results in Example 2, Table I; which, with an increase of 3′ 12″ in the longitude, are assumed in the right hand columns of that table and in Table II, as the place of the meteor at the first Rutland observation. This addition is made to the longitude, because the mean longitudes assumed in Table II exceed a few miles the results from the corresponding Rutland observations in Table I.

A mistake of 1° in the observed altitudes at Wenham would produce an errour of about 2½ miles in the calculated height of the meteor; but the effect of this source of errour cannot be great, since the observed altitude must have fallen between the limits 3° 25′ and 7° 10′, corresponding to the heights of the barn and tree; as was observed above.

With the altitudes, latitudes, and longitudes of the meteor given in Table II, and the latitudes and longitudes of the places of observation, were calculated the distances of the meteor inserted in Table II, to give at one view the results of all the observations.

* This azimuth and altitude were found in the following manner. With the azimuth at Wenham, estimated in Example 1, 79° 33′ 46″, was calculated, by the preceding formula of interpolation, the corresponding altitude at Wenham. The difference between this and the supposed altitude 6° 30′ was called the errour of this supposition. The operation was repeated with another assumed altitude, as 8°, and the errour of this supposition found. By these errours a corrected altitude was calculated by the Rule of False; and by repeating the operation a few times, the above azimuth and altitude were obtained.

TABLE

TABLE I.

Places of the Meteor calculated with various Changes in the Values of the observed Angles for the purpose of estimating the Effect of supposed Errours in those Angles.

Exam.		Azimuths of the meteor at Wenham.	West.	Rutl.	Altitudes of the meteor at Wenh.	West.	Rutl.	Distances of the meteor from Wenh.	West.	Rutl.	Calculated places of the meteor. Alt.	Lat.	Long.	Assumed places of the meteor. Alt.	Lat.	Long
		° ′ ″		°	° ′		° ′	miles	miles	miles.	miles	° ′ ″	° ′ ″	miles	° ′	° ′
1st. Rutland Obs.	1	79 33 46		168½ *	6 30 *		18 27*	120·7		48·1	15·5	42 57 35	73 8 59	18·2	42 51	73 14
	2	83 14 59		168½ *	7 43 *		18 27*	121·8		56·3	18·2	42 51 2	73 10 48			
	3	79 38 1		168 *	6 30 *		18 27*	121·2		48·2	15·5	42 57 31	73 9 29			
	4	77 46 52		168½ *	6 30 *		20 0*	120·6		44·5	15·4	43 0 43	73 8 7			
1st. Wenham Obs.	5	106 54 54*	0° *		5 41 50*	16° 11′		142·2	58·3		16·6	42 3 6	73 27 0	16·7	42 2¾	73 28¼
	6	106 54 54*	3		5 41 50*	16 50		145·2	57·6		17·0	42 2 15	73 30 19			
	7	106 54 54*		170½ *	5 41 50*		7 32	134·6		108·0	15·6	42 5 11	73 18 59	15·7	42 5	73 19½
	8	106 54 54*		173 *	5 41 50*		7 22	129·3		105·6	14·9	42 6 35	73 13 13			
2d Wenham Obs.	9	117 35 54*	0		5 41 50*	35 9		154·6	31·6		18·3	41 37 14	73 27 0	18·5	41 37	73 28½
	10	117 35 54*	3 *		5 41 50*	36 14		156·1	31·2		18·5	41 36 35	73 28 31			
	11	117 35 54*		170¼ *	5 41 50*		6 24	152·2		139·7	18·0	41 38 16	73 25 1	18·1	41 38	73 25½
	12	117 35 54*		173 *	5 41 50*		6 14	144·6		135·1	16·9	41 41 23	73 17 23			
Disappearance at Weston.	13	123 49 42	15 *		5 *	75 *		166·9	18·0		18·0	41 18 52	73 28 23	19·5	41 19⅓	73 28½
	14	123 38 51	15 *		5 30 *	75 *		166·9	20·2		19·5	41 19 20	73 28 33			
	15	123 17 46	10 *		5 *	75 *		165·7	24·6		17·9	41 20 30	73 28 16			
	16	124 17 56	15 *		5 *	80 *		167·4	18·4		18·1	41 17 39	73 27 57			
	17	123 49 42*		172 *	5 *		5 08	161·6		158·5	17·3	41 21 34	73 23 52	19·5	41 19¼	73 29¼
	18	123 38 51*		172 *	5 30 *		5 40	161·3		158·0	18·7	41 22 9	73 23 46			
	19	123 38 51*		170¼ *	5 30 *		5 45	167·1		162·1	19·5	41 19 17	73 29 16			

TABLE II.

Places and Distances of the Meteor at the Times of the different Observations, as determined by the mean of all the Calculations.

Times of Observation.	Distances of the meteor from Wenh.	West.	Rutl.	Places of the meteor. Alt.	Lat.	Long.
	miles.	miles	miles	miles.	North.	West.
At the 1st. Rutland observation.	124	113	57	18·2	42°51′	73 14′
At the 1st. Wenham observation.	139	59	110	16·2	42 04	73 24
At the 2d. Wenham observation	154	32	141	18·3	41 37½	73 27
At the time of disappearing	167	20	162	19·5	41 19½	73 28½

Course of the meteor,

From the places of the meteor given in the preceding table it is easy to find, by the common rules of trigonometry, that *its course was about* S 7° W; *in a direction nearly parallel to the surface of the Earth, and at the height of about eighteen miles.* These points appear to be ascertained to a considerable degree of accuracy. The time elapsed between the disappearance of the meteor, and hearing the three loud reports at Weston, which, according to the estimates of different observers, was *at least* sixty seconds, serves in a degree to confirm the accuracy of the estimated altitude of the meteor. For the velocity of sound being 1142 feet per second, the distance corresponding to 60 seconds is $60 \times 1142 = 68520$ feet, or 13 miles nearly: consequently the height must have exceeded 13 miles.

and its height.

At the first appearance of the meteor at Rutland, it was elevated at least 8° above the horizon of Weston; and at its disappearance at Weston, was above 5° above the horizon of Rutland; as may be easily proved from the places of the meteor given in Table II. Now as it was seen by judge Wheeler and Mr. Page quite near the horizon, it must have been observed at both places from the time of the first Rutland observation till its disappearance at Weston. The distance of the points where the meteor was then situate is easily found from the data in Table II to be 107 miles in a straight line, and *the distance really passed over by the body while visible must have exceeded that quantity.* The whole duration of the appearance of the meteor, as estimated by Mr. Page and judge Wheeler, was about 30 seconds, which

Space passed over by it,

which would make its velocity about $3\frac{1}{2}$ miles per second, by both observations. In a similar manner the distance passed over, while visible at Wenham, was about $52\frac{1}{4}$ miles, and if the duration of its appearance was 30 seconds, as Mrs. Gardner estimated it, the velocity corresponding would be $1\frac{3}{4}$ miles per second; this would have been more than doubled if the extreme azimuths at Wenham had been made use of without correction. From these results it appears probable that *the velocity of the meteor exceeded 3 miles per second.* We may form an idea of the greatness of this velocity, by observing that it is fourteen times as swift as the motion of sound, and nearly as great as that of a satellite revolving about the Earth at the same distance, and if a body were projected in a vertical direction with about double the velocity (the air being supposed not to resist) it would proceed beyond the sphere of the Earth's attraction. and its velocity.

In estimating the magnitude of the body from the present observations, no very accurate result is to be expected, since the apparent diameter was not exactly measured by any of the observers. The observations that were made serve however to prove, that the body was much larger than the whole mass of stones that fell near Weston, as will be evident from the following calculations. Mr. Page supposed the apparent diameter of the body to be about one quarter part of that of the moon, or about 8′. The greatest observed distance of the meteor from Rutland was 162 miles, the least 57 miles. The diameter of the meteor corresponding to those distances and the angle 8′ are nearly $\frac{1}{8}$ and $\frac{1}{3}$ of a mile; and, by this observation, the real diameter of the meteor must fall between those limits. Judge Wheeler supposed the apparent diameter to be half or two thirds of that of the moon, or between 16′ and 24′. The least distance of the meteor from Weston was 20 miles, the greatest 113 miles. The least diameter corresponding to the distance of 20 miles and angle 16′ is $\frac{1}{11}$ of a mile, or rather 491 feet; the greatest diameter, corresponding to the distance 113 miles, and angle 24′ is nearly $\frac{4}{5}$ of a mile, so that the limits furnished by this observation are nearly $\frac{1}{11}$ and $\frac{4}{5}$ of a mile. Mrs. Gardner supposed the diameter to be Its magnitude.

eqnal

equal to that of the moon, or 32′; this, with the extreme distances at Wenham, 124 and 167 miles, furnish the limits 1¼ and 1¼ miles. This last estimate exceeds the others considerably; this may be owing in part to the smallness of the altitude of the object, which probably made it appear larger than it would otherwise have done, from the same cause which makes the moon appear largest when near the horizon. *The least of all the limits of the diameter of the meteor is* 491 *feet.* A body of this magnitude and of the same specific gravity as the stone that fell at Weston (which weighed about 225 pounds to a cubic foot) would contain a quantity of matter exceeding in weight *six millions of tons.* If the specific gravity were the same as that of the air at the surface of the Earth, the quantity of matter would exceed *two thousand tons:* and if the specific gravity were the same as that of the air at the height of the meteor (which by the usual rule for barometrical admeasurements is about $\frac{1}{48}$th part of that at the surface of the Earth) the quantity of matter would exceed *fifty tons.* Either of these estimates exceeds by far the weight of the whole mass that fell near Weston, which, by the accounts published, does not appear to have been greater than half a ton, and would not form a sphere of two feet diameter of the same specific gravity as the stone, as was observed by professor Day, in his valuable paper on the origin of meteoric stones. A sphere of this diameter, seen at the distance of the meteor from Wenham, would hardly be visible without the assistance of a telescope, since its apparent diameter would not exceed two thirds of a second. These reasons seem strongly to favour the opinion, that by far the greater part of the mass continued on its course without falling to the Earth, and the gradual disappearance of the meteor, as observed by judge Wheeler, is agreeable to this hypothesis.

Quantity of matter in it.

The whole apparently did not fall to the Earth.

As it is but within a few years, that observations of these meteors have been carefully made, we have not yet sufficient data for a well grounded theory of their nature and origin; none that has yet been proposed is free from difficulties. The greatness of the mass of the Weston meteor does not accord either with the supposition of its having been formed in our atmosphere, or projected from a volcano of the Earth

Earth or moon; and the striking uniformity of all the masses, that have fallen at different places and times (which indicates a common origin) does not, if we reason from the analogy of the planetary system, altogether agree with the supposition, that such bodies are satellites of the earth.

VII.

On Muriatic and Oximuriatic acid, in Reply to Mr. DALTON. *In a Leter from a Correspondent.*

To Mr. NICHOLSON.

SIR,

MY object in "the remarks on the nature of potassium and sodium, in answer to Mr. Dalton," which you did me the honour to publish in your number for January last, was not so much to advance any thing *new*, as to state fairly the *arguments* used by Mr. Dalton to prove those bodies to be hidrurets; and the *facts*, which induced Mr. Davy to place them in the class of metals. It is not my intention to pursue that subject farther in this communication, as I feel satisfied, that even the splendid talents of Mr. Dalton, (should he honour my animadversions with a reply) will not be sufficient to controvert the deductions drawn by Mr. Davy from his experiments on these bodies—Indeed I should not have troubled you now, had not Mr. Dalton called on me, to explain my data, and mode of calculation, respecting the muriatic acid, and oximuriatic gasses. As to my *data being defective*, and the *principles* of *my calculations unintelligible* to Mr. Dalton, I can only say, that I have taken the former from Dr. Henry's Elements of Experimental Chemistry, 6th edition, 1810, (a work dedicated to Mr. Dalton) where in a table at p. 499, vol. II, I find the specific gravity of muriatic acid gas given as 1·430, according to Brisson—and ·929 according to Kirwan. Here is so great a difference, that, not knowing which to choose, I referred to

Remarks on potassium and sodium.

Principles of the writer's calculations.

Spec. grav. of muriatic acid gas.

to the article "Muriatic Acid" in vol. I, p. 448, where I find its weight stated with respect to common air, as 1·73 to 1·00—Considering this therefore as Dr. Henry's own opinion of its real weight, I assumed it as one basis for my calculation. I am aware that Mr. Davy, at p. 245, Phil. Trans. for 1810, states the weight of 100 cubic inches of muriatic acid gas, at 39 grs. which gives its specific gravity as 1·258—and from the acknowledged accuracy of that gentleman's experiments, I am inclined, on farther consideration, to adopt his estimate, which also nearly agrees with that of Biot and Gay-Lussac. With respect to the specific gravity which I have assigned to oxigen [1·125] I acknowledge an errour I have inadvertently fallen into, from not remarking, that Mr. Davy's estimate of the weight of 100 cubic inches of that gas, viz. 35·06 (vide Henry's Elements, p. 178, vol. I,) is given at the temperature of 55°, and that consequently at 60° it would be but 34.7 (as stated indeed in the same paragraph), which gives its specific gravity, 1·119; and also I will farther allow, that (leaving 1·125 for oxigen) the number 90·72 should be 92·4. I must apologize to *you*, Sir, as well as to Mr. Dalton, for these inaccuracies; which arose from the necessity I was under of putting my thoughts to paper in greater haste than I should have done, had not other avocations claimed the time I would gladly have spent in their revisal. As to my mode of calculation, it is simply this:—Mr. Chenivix's estimate of the composition of oximuriatic gas, (viz. 77·5 muriatic acid +22·5, oxigen) is given by *weight*. To reduce these to proportionate quantities by measure, I divide the specific gravity of the *heavier* gas by that of the *lighter*. I multiply the *quantity* of the *lighter* by the quotient, and add the sum to the quantity of the heavier gas.—And I then say, as the sums so added, are to either of them, so is 100 to the proportional quantity required. Thus $\frac{1{\cdot}73}{1{\cdot}125} = 1{\cdot}537 \times 22{\cdot}5 = 34{\cdot}5 + 77{\cdot}5 = 112$. Then 112 : 34·5 :: 100 : 30·8 = the quantity of oxigen in 100 parts of oximuriatic gas, on the old hypothesis, by *measure*. Consequently this quantity would take twice its bulk of hidrogen, and the whole that would disappear on detonation, would be 30·8 × 3 = 92·4. If *these data* be true, my inaccuracies have been

Spec. grav. of oxigen gas.

Mode of calculation.

The corrected

been rather against my argument than for it; since the corrected numbers give a still larger diminution, than I had before asserted ought to take place. But taking the specific gravity of muriatic acid gas, after Mr. Davy, at only 1·258, and that of oxigen at 1·119, still the diminution ought to be greatly larger, than what really occurs, when 100 parts of oximuriatic, and an equal bulk of hidrogen gasses are detonated together, if the former contained oxigen. In this case, It should be in 200 parts 73·8, a proportion considerably more than ⅓. This indeed approaches to what Mr. Dalton says he should have expected; but how can he reconcile even this difference with the real diminution that happens, and still support the old hypothesis? Whatever data be taken, I think the argument is decisive, in favour of Mr. Davy's views.

Data more favourable to the argument.

I cannot close this communication without expressing my wish, that some philosopher, who has leisure, ability, industry, and *instruments* adapted to the labour, would make the *real weight* of the different gasses, in their *perfectly dry state*, the object of his investigations. It is a subject of material consequence, in many experiments of the refined nature that chemistry now happily begins to admit of, and most desirable, that they should be correctly ascertained; for as the case stands at present, specific gravities of these bodies may be found in our elementary works so widely differing from one another, that a man arguing for victory, rather than truth, may select data almost at will, as best happens to suit his purpose.—That this is not my case, I trust you and Mr. Dalton will readily believe: for I beg to assure that gentleman, that *truth only* is my object, and if he can bring forward *facts*, that prove his arguments correct, I shall be one of the first to acknowledge their justice, and to thank him for having rescued me from error.—I could not else again subscribe myself,

The real weights of the gasses still an important desideratum.

Your, and his humble servant,

JUSTUS.

Feb. 1811.

VII.

VIII.

On Cystic Oxide, a new Species of Urinary Calculus. By WILLIAM HYDE WOLLASTON, *M. D. Sec. R. S.**

New species of stone in the bladder.

THE principal design of the present essay is to make known the existence, and to describe the leading properties of a new species of urinary calculus from the human bladder; but I shall at the same time take the opportunity of correcting an inaccuracy or two, that I have observed in my former communication on this subject. (Phil. Trans. 1797.)

Five kinds before described by the author.

I, on that occasion, took notice of five kinds of urinary calculi,

1. The *lithic acid,* since called *uric acid,* originally analysed by Scheele.
2. The *oxalate of lime,* or *mulberry calculus.*
3. The phosphate of lime, or *bone earth calculus.*
4. The ammoniacal phosphate of magnesia.
5. The *fusible calculus,* which consists of the two last species combined.

Another species since discovered.

It is now about five years since I first met with another species, evidently differing from each of those before described. It was in the possession of Dr. Reeve of Norwich, who obligingly gave me a portion of it for the purpose of examining its chemical qualities. It had been taken from his brother when he was five years old, and at that time was covered with a coating of phosphate of lime very loose in its texture, and consequently very soon separated†. This species is probably very rare: for, although I have omitted no opportunity of paying attention to any urinary concretions to which I could have access, I have, to this time, seen

Very rare.

* Phil. Trans. for 1810, p, 223.

Stone with a hollow centre.

† I am informed, that another stone formed afterward in the bladder of this boy, and that he died in consequence, without submitting to the operation a second time. The stone found in his bladder after death consisted principally of uric acid, but was peculiar in one respect, as its centre was hollow by the removal of some more soluble substance. of which the nucleus had consisted.

only

only one other specimen of the same substance. This last is in a collection of calculi belonging to Guy's Hospital, given by Mr. Lucas, surgeon to that institution, having been formed partly by his father, and partly by himself, in the course of their practice; and according to the present arrangement, (which, it is to be hoped, will not be altered) the calculus to which I allude may be found by reference to No. 46 of that collection. It was extracted by the usual operation from a man of 36 years of age, of whom no record is preserved, except that his name was William Small. It weighed, when entire, 270 grains.

Description of it.

In appearance, these calculi resemble more nearly the triple phosphate of magnesia than any other calculus; but they are more compact than that compound is usually found to be: not consisting of distinct laminæ, but appearing as one mass confusedly crystallized throughout its substance. Hence, instead of having the opacity and whiteness observable in fusible calculi, which consist of a number of small crystals cemented together, these calculi have a yellowish semitransparency; and they have also a peculiar glistening lustre, like that of a body having a high refractive density.

Results of its distillation.

When this substance is submitted to destructive distillation, it yields fœtid carbonate of ammonia, partly fluid, and partly in a solid state, and a heavy fœtid oil, such as usually proceeds from animal substances; and there remains a black spongy coal, much smaller in proportion than is found after the distillation of uric calculi.

Action of the blowpipe.

Under the blow-pipe it may be distinguished from uric acid by the smell, which at no period resembles that of prussic acid; but in addition to the usual smell of burned animal substances, there is a peculiar fœtor, of which I cannot give a correct idea, as I know no smell which it can be said to resemble.

Peculiar smell.

Acted on by most chemical agents.

This species of calculus is so readily acted upon by the generality of common chemical agents, that its character may perhaps be most distinctly marked by an enumeration of those feeble powers, that it can resist.

Exceptions.

It is not dissolved (excepting in very small proportion) by water, by alcohol, by acetic acid, by tartaric acid, by citric acid, or by saturated carbonate of ammonia.

The

Solvents. Acid.

The solvents on the contrary, are far more numerous. It is dissolved, in considerable quantity, by muriatic acid, by nitric acid, by sulphuric acid, by phosphoric acid, and by oxalic acid.

Alkaline.

It is also dissolved readily by pure alkaline meustrua: by potash, by soda, by ammonia, and by lime water. It is even dissolved by fully saturated carbonates of potash or of soda. Accordingly, these alkalis are not so convenient for the precipitation of this matter from acid solutions, as the carbonate of ammonia, which is not capable of redissolving the precipitate, though added in excess.

Precipitates from acids

and alkalis.

For a similar reason, the acids best suited for its precipitation from alkaline solutions, are the acetic and citric acids. But the tartaric acid may occasion an appearance of precipitation, by forming a supertartrate with the alkali employed.

Forms crystals with acids,

The combination of this substance with acids may be made to crystallize without difficulty; and they form slender spicula radiating from the centre, which readily dissolve again in water, unless they have been injured by being in any degree overheated.

The muriatic salt is decomposed by the heat of boiling water, on account of the volatility of the acid, and the rest are easily destroyed by a greater excess of heat.

The salt formed by combination with nitric acid does not yield oxalic acid, and does not become red, as the uric acid does, when similarly treated; but it turns brown, becoming gradually darker, till it is ultimately black.

and with alkalis.

When the combinations with alkalis are evaporated, they leave small granular crystals; but as I was desirous of rendering my experiments as numerous as a limited quantity would permit, the portion which I could employ in any one experiment was too small for me to attempt to determine the form of such crystals.

Precipitated from potash by vinegar

When a hot solution in potash was neutralized by distilled vinegar, the precipitate did not immediately take place, but formed gradually during the cooling of the liquor in minute crystals, some at the surface of the fluid, and others attached to the sides of the vessel. The only definite form, which I could observe, was that of flat hexagonal plates;

in flat hexagonal crystals.

but

but I could discern nothing which enabled me to judge of the primitive form of the crystal. On the surface of the calculus belonging to Guy's Hospital, some minute crystals may be discerned of a different shape, being nearly cubic. And it is possible, that the hexagonal crystals may owe their figure to a small portion of alkali remaining in combination.

Perhaps cubic when pure.

From the ready disposition of this substance to unite with both acids and alkalis, it would appear to be an oxide; and that it does, in fact, contain oxigen, is proved by the formation of carbonic acid in distillation. The quantity of oxigen present in the calculus is not, however sufficient to give it acid properties, for it has no effect on paper coloured with litmus.

Combined with oxigen, but not an acid.

I am therefore inclined to consider it as an oxide; and since both the calculi, that have yet been observed, have been taken from the bladder, it may be convenient to give it the name of *cystic oxide*, which will serve to distinguish it from other calculi; and as this is unlike any other term at present employed in chemistry, it is to be hoped, that it will not be thought to require any alteration.

Cystic oxide.

Since the period of my first essay on gouty and urinary concretions, the general results contained in it have been confirmed by others, and I believe are incontrovertible. But I am under the necessity of acknowledging a mistake in the analysis of the mulberry calculus, though not of much importance. An acid is mentioned to have arisen by sublimation, and it was supposed to originate from a partial decomposition of the oxalic acid. But since pure oxalate of lime yields no such sublimate, it most probably arose from a mixture of the small quantity of uric acid in the calculus then under examination.

Mulberry calculus.

In the analysis of the triple phosphate of magnesia, there is another mistake of more consequence. In my selection from numerous experiments for ascertaining the presence of phosphoric acid, I gave the preference to one in which nitrate of mercury was employed, on account of the facility of extracting the acid from the phosphate of mercury by heat alone. But since the whole of the phosphoric acid is not precipitated by nitrate of mercury, sulphate of magnesia

Mistake in the analysis of the triple phosphate of magnesia

will not be formed on the addition of sulphuric acid, and the magnesia cannot be obtained separate by the same process.

led to another.

It may have been in consequence of this oversight, that a mistake on that subject has occurred in the succeeding volume of the Transactions.

Calculus of a dog supposed to contain lime

A calculus is there described, which had been taken by Mr. Thomas from the bladder of a dog; and a series of experiments are related, from which it was inferred to consist of superphosphate of lime, and phosphate of ammonia. But from the appearance of this calculus (which was exhibited to the society at the time when the paper was read) I was much inclined to think, that the nature of it was mistaken; and upon full consideration of the experiments, they did not appear to me conclusive.

was afterward proved to be a compound of magnesia.

I therefore obtained a portion of the calculus, and, by the following process, the earth contained in it was proved to consist almost wholly of magnesia.

It was dissolved, with the exception of a very small residuum, by distilled vinegar.

The whole of the phosphoric acid was then precipitated by acetate of lead added to excess.

The liquor was then poured off, and sulphuric acid was added, which precipitated the excess of lead, and at the same time formed sulphate of magnesia in solution.

By evaporation to dryness, the acetic acid was removed, and by subsequent increase of heat, the sulphate of ammonia, and excess of sulphuric acid were expelled.

The residuum being then dissolved in water, and the liquor suffered to crystallize by spontaneous evaporation, there remained a quantity of sulphate of magnesia, that weighed rather more than the quantity of calculus taken for the experiment.

Superphosphate of lime perhaps never exists in a calculus.

It was evident, therefore, that in this instance the calculus examined did not consist of superphosphate of lime; and there is some reason to doubt, whether a compound, that is so very soluble in water, ever forms a part of urinary concretions.

Prevention of calculous complaints.

Although the treatment of diseases is not in general a fit subject to occupy the time of this society, there is nevertheless

theless one suggestion, with respect to the prevention of calculous complaints, so nearly connected with my present subject, that I think it may deserve to be recorded.

Uric acid in the dung of birds

Since the white matter contained in the urine of birds, which is voided along with their dung, has been remarked by Mr. Vauquelin, to consist principally of uric acid, I have paid some attention to the different proportion in which this matter is voided by different species of birds, to see how far it accorded with the different qualities of their food.

seems proportional to the quantity of animal food they take,

And I found, that in the dung of the goose, feeding wholly on grass, the proportion did not seem so much as $\frac{1}{200}$ of the whole dung. In that of a pheasant kept in a cage, and fed on barley alone, it was about $\frac{1}{11}$ part. In that of a hen, having the range of a garden and farm yard, and consequently procuring insects, and possibly other animal food, the proportion was manifestly much greater, and combined with lime. In the dung of a hawk, fed upon flesh alone, the quantity of matter voided in a solid state bears but a small proportion to the residuum of uric acid, that is left by the urine when dry. And in the gannet, feeding solely on fish, I have observed the evacuations in some instances to be mere urine, for it contained no solid matter excepting the uric acid.

particularly of fish.

Hence calculous and gouty persons should prefer a vegetable diet.

It seems, consequently, deserving of inquiry, what changes might be produced in the urine of any one animal, by such alterations of diet, as its constitution would permit; for as far as any inference can be drawn from these varieties, which naturally occur, it would appear, that persons subject to calculi, consisting of uric acid, as well as gouty persons, in whom there is a redundance of the same matter, have much reason to prefer vegetable diet: but that the preference usually given to fish above other kinds of animal food is probably erroneous.

IX.

Comparative Analysis of Gum Resins: by Mr. HENRY BRACONNOT, *Professor of Natural History, &c.*

(Continued from vol. XXVII, *p.* 370.*)*

ART. II. *Analysis of Gamboge.*

Effects of heat on gamboge.

Sect. I. IF gamboge be exposed to the flame of a candle, it swells up; and burns like a resin. Heated in a capsule it emits a peculiar smell, softens, and is decomposed before it melts.

Products of its distillation.

Fifty gram. [772 grs.] exposed to distillation produced

1st, a brown water, containing empyreumatic acetic acid:

2d, a small quantity of a light oil:

3d, afterward came over in considerable quantity another oil, heavy, thick, and brown.

Coal.

In the retort remained a light coal, weighing 8 gram. [123·5 grs]. It was incinerated with difficulty, and left 5 decig. [7·7 grs] of ashes, which yielded 2 cent. [0·3 of a gr.[of potash in part sulphated, 4 cent. [0·6 of a gr.] of phosphate of lime, 6 cent. [0·9 of a gr.] of carbonate of lime, and 3 dec. [4·6 grs] of quartz sand, containing a little charcoal, and some traces of oxide of iron. No ammonia was found in the liquid products.

Gamboge treated with alcohol,

Sect. II. Twenty gram. [309 grs] were treated with hot alcohol and filtered. What remained on the filter, after being well washed with alcohol, was a grayish substance, that dried with difficulty, and was then brittle. In this state it weighed exactly 4 gram. [61·7 grs]; had a faint taste, or was nearly insipid; and dissolved entirely in water, except a decig. [1·5 gr.] of impurities. The solution reddened litmus. Evaporated to dryness it left a transparent, friable residuum, resembling the high coloured gum of the plum-tree, burning like it with little flame, and leaving a considerable quantity of a compact coal, in which was some phosphate of lime.

The alcoholic solution was red. Evaporated to dryness it

it afforded 16 gram. [247 grs] of resin. This was transparent, red, without any perceptible taste, and pretty decidedly electric by friction. When powdered it emitted a peculiar smell, and assumed a bright yellow colour.

On pouring water into a saturated solution of this resin in alcohol, there is a sensible evolution of heat, and a uniform, yellowish, milky liquid is produced; while most other resins are precipitated from alcohol in clots. precipitated by water,

Sect. III. Solution of potash acts very quickly on the resin of gamboge, particularly if heated. The result is a red liquid of an oily appearance, in which the properties of the potash are neutralized. On evaporating this compound almost to dryness, it crystallizes like the solution of aloes. and acted on by potash.

The soap or saponule of this resin is of a deep red approaching to black, and feels greasy between the fingers. When dried it is friable, and resembles a resin. It has the taste of rancid fat, leaving a slight sensation of acrimony at the root of the tongue. It is easily soluble in water, without rendering it turbid. Acids precipitate this solution in such abundance, that the whole becomes a thick coagulum of a fine yellow colour. Lime-water throws down from it a fine orange coloured precipitate. Earthy salts and most solutions of the white metals likewise produce yellow precipitates in it. It precipitates sulphate of iron brown, and nitrate of copper green. Soap from it.

Sect. IV. Ten gram. [154·4 grs] of the resin of gamboge were put into a retort with 80 parts of strong nitric acid of the shops. As soon as the retort felt the heat of the fire, red vapours arose, the intensity of which soon disappeared. The first product was returned into the retort, and the operation continued, till the matter was dissolved, and the solution reduced to the consistence of a sirup*. On cooling a mass of lamellar crystals was formed, enveloped in a vis- The resin treated with nitric acid.

* The product of this distillation, being thoroughly saturated with chalk, and distilled anew, yielded a slightly acid liquor, of a very pungent smell, and bitter to the taste. Alkalis gave it a light yellow tinge. On adding sulphate of iron to this mixture a precipitate was formed, which was completely soluble in acids; so that there was no prussic acid in this liquid, but I am not fully acquainted with its nature. Product of the distillation.

cid

cid matter. The whole being diluted with a quantity of water, a sediment was formed, which, when well washed and dried, weighed 1·3 gram. [20 grs].

Product. This matter is of a yellowish colour, and bitter taste. It is partly soluble in boiling water. On cooling the solution grows turbid, and lets fall a portion. The filtered solution is of a reddish yellow, froths when shaken, reddens infusion of litmus, is rendered of a deeper colour by the addition of an alkali, and forms a slight precipitate after some time with sulphate of iron.

On burning coals this substance does not melt so easily as the resin, diffuses a fragrant smell, and leaves a great deal of coal.

It combines very well with potash and spirit of wine, forming with them transparent red solutions.

Nitric acid, heated gently with it, dissolve it, without occasioning any sensible alteration. Water produces a copious white coagulum in the solution.

A resinoamer. From these properties I think I may fairly consider this substance as a particular species of soluble factitious resinoamer, combined with a yellowish resiniform substance insoluble in water.

Produce of the mother waters. The acid liquor and waters of elutriation were boiled down, to drive off any nitric acid that might remain; and the residuum was diluted with water, in which a small quantity of potash was dissolved, which separated 4 dec. [6 grs] of the yellow resiniform substance. The liquor being again boiled down, then treated with alcohol, and filtered, left 1 gram. [15·4 grs] of very white superoxalate of potash. The alcoholic solution produced on evaporation 3 gram. [46·3 grs] of bitter matter, soluble in water, and containing malic acid.

The resin acted on by oximuriatic gas. *Sect.* V. I diluted some of the resin of gamboge in fine powder with water; and passed into it a stream of oximuriatic acid gas, to try its effect on the colour; and in fact it destroyed its fine yellow. The milky liquor being boiled down, then diluted with water, and filtered, left on the filter a substance, which was washed with boiling water, till what came off would no longer redden litmus. The following were its properties.

It

It is pulverulent, of a pale yellow colour, and without any perceptible taste. It crackles between the teeth like an insoluble salt, and boiling water will not dissolve it. It is very little fusible, and emits no smell, till it begins to be decomposed; but if it be set on fire, or thrown on burning coals, it emits pungent fumes of muriatic acid. Properties of the product.

Weak acids separate nothing perceptible from it; but with concentrated acids charcoal and muriatic acid are produced.

Combined with potash the compound has a pleasant soapy smell, and nitrate of silver throws down from its solution a precipitate partly soluble in nitric acid.

I distilled 6 gram. [92·6 grs] of this substance in a small retort, which was heated to redness. The product was collected in a few decigr. [about $\frac{1}{4}$ of an oz measure each] of water, which, being examined toward the end of the distillation, was very sour, and had the smell of muriatic acid. To this water I added nitrate of silver, which produced a copious curdy precipitate of muriate of silver. This precipitate weighed 5·4 gr. [83·4 grs], which would contain 1·35 gram. [20·84 grs] of muriatic acid, according to the proportions given by Bergman of 25 acid to 75 oxide*. Distilled.

In the retort were left 2·1 gram. [32·4 grs] of a tumid coal.

Hence it follows, that 100 parts of this acidiferous resinous substance were composed of

Its component parts.

Dry muriatic acid	22·5
Charcoal	35
Oxigen, hidrogen, and carbon	42·5
	100

Sect. VI. Thus it appears, that gamboge is a true gum-resin, since we find in it a peculiar resin very well marked, and a gum resembling that of many of our fruit trees. Gamboge a true gum-resin.

* This certainly estimates the acid too high. From 19 to 19·5 per cent of acid is the most that can be allowed, according to the experiments of several of the most eminent modern chemists. C.

ART.

Art. III. *Analysis of Euphorbium.*

Euphorbium from different plants.

Sect. I. The Dutch have their euphorbium from Malabar, where it flows from the euphorbia antiquorum; that used in England is from the euphorbia canariensis*, which furnishes the euphorbium used in France, as Mr. Braconnot had an opportunity of satisfying himself, by finding several branches of the tree among the specimens he examined.

Action of heat on it.

Sect. II. Euphorbium, when exposed to a gentle heat, softens easily, and loses a twentieth of its weight in moisture.

Boiled in water.

I boiled 4 gram. [61·8 grs] in 100 gram. [1544 grs] of distilled water. The filtered liquor left an insoluble substance,

Properties of the solution.

which, when dried, weighed 3 gram. [46·3 grs.]. What passed through was of an amber colour, and had a bitter taste with a slight degree of acrimony.

This solution reddened infusion of litmus. Oxalate of potash threw down from it a pretty copious precipitate of oxalate of lime. Nitrate of lead formed in it a white precipitate entirely soluble in distilled vinegar. Lime-water rendered it turbid, and occasioned a yellow precipitate, which vinegar dissolved.

Boiled in alcohol.

Sect. III. *A.* I boiled 20 gram. [308·8 grs] of euphorbium in 90 gram. [1389·6 grs] of alcohol at 36° [spec. grav. 0·837], which was sufficient to dissolve all the parts capable of solution. This solution filtered at a boiling heat left a substance on the filter, which, after being well washed with alcohol and dried, weighed 6·4 gram. [98·8 grs.]

Deposit from the alcohol on standing,

B. Having mixed together the alcoholic solutions, which had grown turbid on cooling, and let them stand at rest for two days, a considerable quantity of a white, granular, and somewhat gelatinous substance was deposited, which, being washed with alcohol and dried, weighed 4·7 gram. [72·5 grs]. It still retained some alcohol, which being driven off by heat, only 3·4 gram. [52·5 grs] remained.

similar to wax.

This substance was semitransparent, capable of being indented by a hard body, softened readily between the fingers, was almost wholly volitalized on a redhot iron, and com-

* According to our college, who have readmitted it into their last Pharmacopœia, from the euphorbia officinarum. C.

ported

ported itself like bees wax, which it resembles in smell when melted or burned. This wax of euphorbium retained a slight degree of acrimony, no doubt because it had not been sufficiently washed with alcohol. I made a taper of it, which burned with a very clear flame.

C. The 6·4 gram. [98·8 grs] insoluble in alcohol (*A*) were heated to the boiling point in 100 gram. [1544 grs] of distilled water. The filtered liquor left behind little bits of wood and thorns, which when dried weighed 2·7 dec. [4·17 grs]. The matter insoluble in alcohol boiled in water

D. The aqueous solution (*C*), being evaporated, formed a varnish on the surface of the glass. On evaporating to dryness a brittle substance was obtained, which separated in micaceous scales, did not attract moisture, and weighed 4·1 gram. [63.3 grs], which I perceived at once to be malate of lime*. In fact, on heating this substance with diluted sulphuric acid, I obtained, 1st, very white sulphate of lime, which, after being washed and dried, weighed 1·6 gram. [24·7 grs]: 2dly, an acid, which alcohol dissolved, and from which it separated 5 dec. [7 grs] of sulphate of lime. The solution, being evaporated, produced 2 gram. [30·83 grs.] of malic acid, retaining a little sulphuric acid, which was separated by barytes. and evaporated, yielded malate of lime.

Malate of lime therefore appears to exist in tolerable quantity in the milky juice of the euphorbias; and it was this salt, which the older chemists mistook for a gum in the euphorbium of the shops, and which Mr. Laudet has confounded with extract. This mistaken for gum and extract.

E. The alcoholic solution (*B*), being evaporated to dryness, left a residuum weighing 8·3 gram. [128 grs]. This was treated with cold alcohol, which dissolved the resinous parts, and farther separated 4 dec. [6·2 grs.] of wax. More wax obtained.

F. This solution being evaporated afresh, a resin was obtained, that attracted moisture from the air in a small degree. This was owing to the presence of malate of potash, which I obtained by heating the resin with distilled water. When well dried it weighed 4 dec. [6·2 grs.] Resins mixed with malate of potash.

* I have already made known, that the ricinus, which is of the family of euphorbias, contains malic acid, neutralizing a large quantity of potash and lime.

G. The

Properties of the resin.

G. The resin of euphorbium has a reddish transparency, and extreme acrimony, whence it may be considered as a violent poison. It becomes electric by friction. Alkalis have no sensible action on it. Sulphuric acid dissolves it cold. Treated with nitric acid at a heat of 20° [77° F.] it softens, grows yellow, and begins to decompose. By increasing the heat, a complete solution is obtained, which, when evaporated, yielded a great deal of yellowish resiniform matter, a soluble resinoamer substance, and some traces of oxalic acid.

Component parts of euphorbium.

Sect. IV. From these experiments it follows, that 100 parts of euphorbium are composed of

Water	5
Wax	19
Woody matter	13·5
Malate of lime	20.5
——— potash	
Resin	37
Loss	3
	100

Art. IV.—*Analysis of Myrrh.*

Myrrh distilled alone.

Sect. I. Thirty gram. [463·2 grs.] of myrrh gradually heated to redness in a retort yielded 10 gram. [154.4 grs.] of an empyreumatic, heavy, brown oil; 10 gram. [154·4 grs.] of a red liquid, that changed sirup of violets green, yielded ammonia when mixed with potash, and acetate of potash mixed with oil when evaporated.

Coal.

The coal left in the retort occupied less space than the myrrh itself, was compact, iridescent, shining, and weighed 7·25 gram. [112 grs.] Being icinerated it produced 1·6 gram. [24·7 grs.] of white ashes, which furnished by elutriation 7 cent. [1.08 grs.] of sulphate of potash mixed with a small quantity of subcarbonate.

The part of the ashes insoluble in water dissolved completely in nitric acid with effervescence, carbonic acid being evolved mixed with a little sulphuretted hidrogen, arising no doubt from the decomposition of a small quantity of sulphate of potash by the lime and carbon. The nitric solution

lution contained nothing but lime, which was separated entirely by subcarbonate of potash.

Sect. II. *A.* Fifty gram. [772 grs.] of myrrh distilled with water yielded a product having the smell of the myrrh, and in which floated a little volatile oil. Myrrh distilled with water.

B. The residuum of this distillation being thrown on a filter, the liquid was a long time in passing through. The matter that did not dissolve was well washed with boiling water.

C. The several liquors, being mixed and evaporated to dryness, left 23 gram. [355 grs.] of a red transparent gum, bitter to the taste. Gum.

1. This gum reddened infusion of litmus. Its properties.

2. Treated with boiling water it dissolved only in part. What remained was a substance of a gummy appearance, and was perfectly insoluble even in dilute acids. It was bulky, became brittle when dried, and afterwards swelled up in boiling water without dissolving. This insolubility acquired by the gum of myrrh appears to be owing to heat.

3. The gum of myrrh yielded by distillation subacetate of ammonia, some oil, and a coal, which incinerates very easily compared with those of animal matters.

4. Treated with dilute nitric acid, and gently heated, it gave out carbonic acid gas mixed with nitrogen; a yellow flocculent matter was deposited, which soon disappeared; and, the liquor being evaporated, oxalic acid remained, mixed with malic acid, and a bitter yellow matter, which did not detonate.

5. Oxalate of potash threw down oxalate of lime from the solution of the gum.

6. Lime-water in excess did not alter its transparency, so that it contained neither malic nor phosphoric acid.

7. Neither decoction of galls nor oximuriatic acid occasioned any change in it.

8. Several metallic solutions, as those of lead, mercury, and tin, form copious white precipitates in the aqueous solution of this gum.

The precipitate produced by nitrate of lead in 23 gram. [355 grs.] of this gum dissolved in water weighed 20·2 gram. [311·8 grs.]. It was of a reddish colour, like myrrh, and semitransparent

semitransparent. Boiling water only divided it; but, on adding gradually weak sulphuric acid to a very slight excess, the clots disappeared, and it was filtered. What remained on the filter, when well washed and dried, consisted of 5·3 gram. [81·8 grs.] of sulphate of lead, containing 4 gram. [61·76 grs.] of oxide; which had been combined with 16 gram. [247 grs.] of the gum, that were reobtained by evaporating the liquor separated from the sulphate of lead. This gum, which retained an excess of acid, yielded neither malic nor phosphoric acid when treated with alcohol; which led me to suspect, that the lime contained in the gum of myrrh is saturated by acetic acid; and perhaps also by carbonic; for, if sulphuric acid be poured into a solution of this gum, a slight effervescence takes place, and a precipitate of sulphate of lime.

We see from what precedes, that the gum of myrrh is not entirely separated from its solvent by the nitrate of lead; since of the 23 gram. [355 grs.] only 16 [247 grs.] fell down with the oxide. This must have been owing to the nitric acid set free; for a salt of lead surcharged with oxide, as the subacetate, precipitates almost wholly the solution of the gum of myrrh, and entirely, if a little alkali be added to the mixture.

The part insoluble in water treated with alcohol.

D. What remained on the filter (*B.*) was heated with alcohol, which dissolved all the resinous parts, and left behind a soft, transparent, substance, insoluble in boiling water, and weighing when dried 6 gram. [92.6 grs.]. It had all the properties of the gummy matter already mentioned.

Resin produced.

E. The alcoholic solution (*D.*), mixed with the spirit used in washing the filters, yielded on evaporation 11·5 gram. [177·6 grs.] of a brown resin, with an aromatic bitter taste like that of myrrh.

Its properties.

1. This resin softens easily between the fingers, melts at 48° R. [140° F.], and does not become electric.

2. It emits an aromatic smoke while burning, and yields on distillation the same products as resins.

3. With potash it forms a kind of soap, the aqueous solution of which passes turbid through the filter.

4. On 5·5 gram. [85 grs.] of this resin of myrrh were poured

poured 33 gram. [509.5 grs.] of nitric acid at 38°, which turned the resin blackish. The mixture being distilled emitted red vapours, but not very abundantly. After having obtained a product of about 20 gram. [309 grs.] from this solution, the retort was removed from the fire. A resiniform substance of an orange colour floated in it, which, when washed and dried was of a pale yellow, and weighed 1·5 gram. [23 grs.]. It was pulverulent, bitter, not very fusible, partly soluble in water, and formed with potash a saponaceous compound, which dissolved very easily in water, giving it a red colour, without any diminution of its transparency. This resiniform substance contains a great deal of carbon, and is sensibly altered by nitric acid, which only dissolves it. In other respects it comports itself like the resin of gamboge treated with nitric acid.

The nitric solution, on which this substance floated, being evaporated to dryness, left a residuum, which being well washed, furnished 1 gram. [15·4 grs.] more of the resiniform substance, which had been kept dissolved by means of the nitric acid. Lime-water in excess added to the waters of elutriation separated 1·2 gram. [18·5 grs.] of oxalate of lime mingled with a small portion of malate. The supernatant liquid contained a bitter yellow matter.

Sect. III. From these experiments it follows, that myrrh consists principally of a gum different from the common kind, the leading properties of which are

Myrrh consists chiefly of a gum.

1, To acquire a degree of cohesion from the action of heat, when its solutions are evaporated, which renders it partly insoluble in water:

Its chief characteristics.

2, To give out ammonia when distilled, and nitrogen gas when acted on by nitric acid; which gives it an affinity to animal substances:

3, To decompose solutions of lead, mercury, and tin, and unite with their oxides.

Myrrh contains likewise about 0·23 of its weight of a very fusible bitter resinous matter.

(To be continued.)

SCIENTIFIC

SCIENTIFIC NEWS.

Royal Society of Edinburgh.

Royal Society of Edinburgh.

ON the 7th of January Sir George M'Kenzie continued his account of the Mineralogy of Iceland, and described some very curious geological facts.

On the 21st he concluded his mineralogical detail, with an interesting description of Mount Hecla and other volcanic districts. In this paper Sir George made some remarks which tended to place obsidian and pumice in a conspicuous point of view, as relating to the different theories of the Earth, and clearly proved their origin to be igneous, a position which has hitherto been denied by Werner and his pupils.

On the 4th of February Dr. Brewster read an ingenious paper on the loss of the comet of 1770.—Sir George M'Kenzie described some remarkable hot springs in Iceland. To one of these he gave the name of the alternating *Geyser*, as it spouted from two distinct orifices evidently connected within, but only from one at a time, the operations of which alternated with those of the other at regular intervals of time.

On the 18th Professor Playfair read part of a Biographical Memoir of the late John Robison, LL.D. and Professor of Natural Philosophy in the University of Edinburgh. Mr. Allan communicated a letter from Dr. Henry, of Manchester, describing the position of some singular masses of a substance apparently composed of wax and resin, which had been laid bare by a late overflow of the river Mersey, a little below Stockport, about three feet under the soil, and supposed to be the refuse of some manufactory, of which no other vestige or recollection now remains.

Geological Society.

The First Volume of the Transactions of the Geological Society, in 4to, with many plates, is in the press, and will be ready for publication in the month of May next.

TO

TO CORRESPONDENTS.

Philochemicus does not seem to be aware, that the object of the galvanic troughs he mentions was to get rid of the cement, all the disadvantages of which recur in the plan he proposes. Any danger to be apprehended from the use of earthenware or glass may readily be obviated by the simple and cheap defence of a case of common wood.

With regard to the use of the word particle, when he defines it, in one place, the smallest portion of any compound substance into which it can be resolved without decomposition; and, in another, the smallest portion of any compound body, which we can procure by mechanical means; these two definitions are obviously not consistent with philosophical precision. The fact appears to be, that the word particle, or small part, a word with a certain laxity of signification in common use, cannot well therefore be confined to one precise and definite meaning on all occasions; but like many other terms, will have its exact sense to be determined in general by the context.

L. O. C.'s paper will be inserted the earliest opportunity.

As a request formerly made appears not to have been seen, or to have been overlooked, by some of my more recent correspondents, I beg leave to repeat it. It is, that they would leave in their manuscript a margin for the insertion of the side notes. It is also desirable, that, when a paper requires drawings for its illustration, none of it should be written on the back of the drawings.

METEOROLOGICAL JOURNAL,

For FEBRUARY, 1811,

Kept by ROBERT BANCKS, Mathematical Instrument Maker, in the STRAND, LONDON.

JAN. Day of	THERMOMETER. 9 A. M.	9 P. M.	Highest in the Day	Lowest in the Night.	BAROMETER, 9 A. M.	RAIN, noted at 9 A. M.	WEATHER. Day.	Night.
28	30·5	30·5	33°	24°	29·61		Fair	Fair
29	27	29	31·5	23	·60		Ditto	Ditto
30	27	33·5	33·5	33.5	·73		Ditto	Cloudy*
31	37	42	43·5	33	·16		Rain	Rain
FEB.								
1	39	40	42·5	35·5	·22	·810	Ditto	Ditto
2	40	33	44·5	40	·62	·145	Fair	Fair
3	45	40	47	33	·33		Ditto	Ditto
4	36·5	37	44	34	30·08		Ditto	Ditto
5	37·5	41	42	38	·05		Cloudy	Ditto
6	44	46	47	42	29·61		Ditto	Ditto
7	45	44	47	39	·60		Rain	Ditto
8	44	47	48·5	39	·70	·675	Ditto	Cloudy
9	42	45	49	41	·60	·015	Ditto	Ditto
10	46·5	50	51	41·5	·82	·020	Ditto	Rain
11	48	49	50·5	38	·54	·030	Ditto	Ditto
12	48	46	49	34	·30	·100	Ditto	Fair
13	38	38	40	33	·32	·010	Ditto	Ditto
14	39	40	42	34	·26	·050	Fair	Foggy
15	37	40	41·5	34	·68		Rain	Rain
16	39	35	41	28	·44	·210	Fair	Fair
17	32	36	37	33	30·24		Cloudy	Ditto
18	37	38	41·5	31·5	·13		Fair	Ditto
19	35	35	39·5	32	29·97		Ditto	Ditto
20	36	40	40	36	·73		Ditto	Foggy
21	40	44·5	44·5	40	·47		Rain	Rain
22	44	42	47	38	28·97	·060	Ditto	Fair
23	42	44	46·5	40	29·20	·060	Ditto	Cloudy
24	45	44·5	47·5	36	·13	·190	Ditto	Rain
25	41	44	48	40	·26	·080	Fair	Fair†
26	49	46	52	40	·21	·180	Rain	Ditto

2·645 Inch. since last Journ.

* Heavy snow in the night and Thaw. † Rain in the Night.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

APRIL, 1811.

ARTICLE I.

Observations and Experiments on the Alkaline Metalloids: by J. MURRAY, *Lecturer on Chemistry, &c., Edinburgh.*

To Mr. NICHOLSON.

SIR,

THE speculation with regard to the nature of the metallic bases of the alkalis, which I venture to submit to your readers in the following pages, occurred to me some months ago, on perusing a paper by Berthollet in the 2d volume of the Mémoires D'Arcueil, on the existence of combined water in potash. Having only lately had it in my power to perform the experiments connected with the investigation, I now transmit to you a general account of the results, which may not perhaps be without interest in the present state of chemical inquiries.

Berthollet's remarks on water in potash,

In the memoir to which I allude, Berthollet relates experiments, which appear to prove, that potash prepared by the usual process contains a considerable proportion of water, from which, while it exists in an insulated state, it cannot be freed by the application of heat. The heat at first

which cannot be expelled by heat.

first dissipates a portion of the water; but in continuing the application of it, the potash itself rises in vapour before the whole of the water is expelled. Hence Berthollet found, that potash in this state yields, when it is mixed with iron filings, and exposed to a sufficiently elevated temperature, a large quantity of hidrogen gas. From this it appears evidently to contain water; and from the results of its combination with muriatic acid he infers, that the quantity amounts to 13·64 in 100 parts of potash which has been kept in fusion until it rises in vapour.

This apparently incompatible with the theory of the decomposition of potash by galvanism,

Admitting this, it appears to lead to conclusions incompatible with the theory, which has usually been received, of the decomposition of potash by galvanism. In this decomposition oxigen is given out at the positive side, and potassium appears at the negative side; and according to the theory given by Mr. Davy of this experiment, this is owing to the decomposition of the alkali—the potassium is its base, and the oxigen the principle, with which this base had been combined.

as the water should be decomposed, but no hidrogen is evolved.

But if the existence of water in potash be admitted, the results are not conformable to this theory. This water, there can be little doubt, must suffer decomposition from the action of galvanism upon it, its oxigen will be evolved at the positive pole, and its hidrogen ought to appear at the negative side. No such evolution of hidrogen however is to be observed in the common mode of making the experiments; and Mr. Davy, by whom it has been executed with so much care, and on so large a scale, has remarked, that at the negative surface there is no liberation of elastic fluid, nothing being produced there but the metallic globules. It appears therefore to follow almost as a necessary conclusion, if the existence of water in potash be established, that, since hidrogen does not appear in its insulated form, it must enter into the composition of the metallic matter, which appears at the negative side; and of course a different view must be given of the operation from that which regards it as a mere decomposition of the alkali.

Simplest mode of investigating this point.

There are several modes of investigation, by which the inquiry with regard to this may be prosecuted. The simplest experiment however, by which light may be thrown

on

on it; and which, as it appeared not difficult to execute with accuracy, I preferred; is to ascertain the nature of the product of the oxigenation of potassium. It was ascertained by Mr. Davy, that, when potassium combines with oxigen, it forms solid, and apparently unusually dry potash. Now if potassium is simply the base of the alkali, forming it when united with oxigen, the potash formed in this way must be the pure alkali; there is no source whence any water can be communicated to it, and of course it cannot contain that portion of water, which is essential to the potash prepared by the common process. But if it be found to contain water, it may be inferred, that hidrogen had existed in the composition of potassium, which, combining with oxigen during the combustion, formed this water.

Mr. Davy's calculation of the dryness of potash formed from potassium,

Mr. Davy had perceived the importance of the inquiry with regard to the presence of water in potash, and endeavoured to determine the relation of the dryness of the potash formed from potassium to that existing in some of its saline combinations supposed to be free from water. Berthollet had inferred, that potash which had been in fusion parts with 13·89 of water when it enters into combination with muriatic acid. By converting potassium into muriate of potash in muriatic acid gas, Mr. Davy obtained results, whence he inferred, that the alkali, which Berthollet had taken as a standard, contains 9 per cent more water than that existing in the potash formed by the combustion of potassium in muriatic acid gas, so that the fused potash of Berthollet contains consequently near 23 per cent of water; and he adds, that, from his own observations, potash kept for some time in a red heat contains 16 or 17 per cent of water, taking the potash formed by the combustion of potassium as the dry standard.

liable to uncertainties.

These conclusions however become uncertain in consequence of the different theory, which Mr. Davy has proposed on the action of muriatic acid gas on potassium; they are liable too to the uncertainties, which arise from the difficulty of estimating the quantity of water in muriatic acid, and the subject must be investigated under another form.

Comparison of the neutralizing powers

The following appeared to me to be the simplest mode of experiment, that in which the conclusion is most direct, and

of fused potash and potash from potassium.

and the investigation least liable to errour—to determine if there is the same quantity of real alkali in the potash formed by the oxigenation of potassium, and in the potash prepared by the usual process, and which has been kept in fusion, by comparing their powers in neutralizing an acid.

The difference very trivial.

I dissolved 10 grains of pure potash prepared by the usual process, which had been kept in fusion in a low red heat, in half an ounce of water, and neutralized this by the addition of nitric acid previously diluted with 20 parts of water: 165 grains of this diluted acid were required for neutralization. The dry product of the combustion of potassium was then dissolved in water, there remained undissolved a minute portion of matter—On removing this and weighing it, the remaining solution required for neutralization of the above diluted acid a quantity which was equivalent, t a mean of several experiments, to 169·5 grains to 10 grains of the product, supposing it to have been free from any any intermixture of insoluble matter. Here then the same quantity of acid was required for neutralization as nearly as can be expected, for the difference of 4·5 grains of an acid so much diluted on so large a quantity as 169 grains is far from being equal to the difference, which ought to have been found, were the product of the combination of potassium the real alkali, free from water; and is indeed so trivial, that it may nearly be neglected.

Water to be expected in the potash from potassium.

That this would be the case might nearly be inferred a priori; for in decomposing potash by galvanism, there is no appearance of the water which it contains being dissipated. Either that water therefore in its entire state, or its base, must exist in the product; its oxigen, according to the latter supposition, being given out; and of course when that product is again converted by oxigenation into potash, the water must be found in this potash.

Desirable to heat it with iron filings.

An interesting experiment in confirmation of the above results would be to submit the dry product of the oxigenation of potassium to the experiment made by Berthollet on common potash—heating it with iron filings to ascertain if it affords hidrogen. This I intend to do, when I have procured a sufficient quantity of potassium to admit of the experiment being made with accuracy.

From

From the present investigation it appears to be established, that potassium contains hidrogen. This rests on two facts; *first*, that potash, which is proved to contain water, gives out no hidrogen, when converted into potassium by galvanism; and *secondly*, that potassium in combining with oxigen forms potash containing water.

Potassium apparently contains hidrogen.

Gay-Lussac and Thenard, soon after the publication of Mr. Davy's discovery of the metallization of the alkalis, proposed a different theory of the process from that which he had advanced. They considered the new metals as compounds of the alkalis with hidrogen. Mr. Dalton has embraced this view, partly from the consideration of the presence of water in fused potash, and partly from the consideration of the levity and volatility of potassium. The facts above established, it may be supposed, are in conformity to this theory. They may no doubt be explained by it, while they seem not reconcilable to the hypothesis of Mr. Davy. I would propose however a different view, equally adapted to these phenomena, and as it appears to me more conformable to analogy.

Hypotheses of Gay-Lussac, Thenard, and Dalton.

I would suppose with Mr. Davy, that, in the production of potassium and sodium, the potash and soda are decomposed; the oxigen, which I also suppose combined with them in these alkalis, being disengaged at the positive side. But instead of regarding them as the real bases of the alkalis, I would consider them as compounds of these bases with hidrogen. While the alkali is decomposed by the galvanic action, the water it contains must equally be decomposed; the oxigen of both will, in conformity to the law which regulates galvanic decomposition, be attracted to the positive side, and at the negative side the metallic base of the alkali will be evolved; and the hidrogen of the water, being there also in conformity to the same law disengaged, will in its nascent state combine with that base. The potassium or the sodium is the product of this combination. The theory of the production of these substances in the decomposition of the alkalis by iron at a high temperature, in conformity to these views, must be obvious—the iron attracting the oxigen both of the alkali, and of the water, and the hidrogen of the water combining with the real metallic

Mr. Murray's.

The metalloids formed by the intermedium of iron probably contain a smaller propor-

tion of hidrogen.

metallic base, the potassium obtained by this process has been observed to have a greater specific gravity, than that obtained by the action of galvanism. This has been ascribed to a slight alloy of iron, which has not however been detected. It is not improbable, that it may be owing to its containing less hidrogen, a quantity of hidrogen being disengaged in the process by which it is formed, while no evolution of this principle is observed in the production of potassium by galvanism. The whole hidrogen of the water therefore, which existed in the potash, will not be contained in the potassium formed in this way; and to this may be owing that slight superiority of alkaline strength, observed in the above experiments, of the potash formed from potassium.

One or other of these theories—either that proposed by Gay-Lussac and Thenard, or that which I have stated, it appears to me must be adopted; and the latter farther appears to me in several respects preferable to the former.

Arguments in favour of the author's hypothesis.

There is no analogy in favour of the assumption, that the alkaline metals are compounds of the alkalis with hidrogen, but rather an improbability. There are no grounds whence we can infer, that the common metals are compounds of what we at present regard as their oxides with hidrogen; and unless we make this assumption, there is an improbability in the hypothesis, that the alkaline metals, which have all the metallic properties, are compounds of this kind.

An analogy can be traced connecting the earths with the common metallic oxides, and a similar analogy connects the alkalis with the earths. This leads to the conclusion, that all these substances are of similar chemical constitution; that the earths and the alkalis therefore, like the metallic oxides, are compounds of metallic bases with oxigen.

The hypothesis I have advanced is in conformity with both these analogies; it has therefore all the advantages, which belonged to the theory given by Mr. Davy; and, independent of the above facts, which appear to be subversive of that theory, it has some advantages which Mr. Davy's cannot claim. In particular the levity of the alkaline

line metalloids, which has always been regarded as an anomaly, may be supposed to arise from the presence of hidrogen.

That these substances should retain metallic properties, supposing them to be metals combined with hidrogen, cannot be regarded as an objection. Inflammables in uniting with metals do not altogether subvert, and frequently even do not materially modify the metallic properties. Steel, the compound of iron and carbon, has all the qualities of a metal. Sulphuret of iron has the lustre, opacity, and hardness of a metal; and sulphuret of lead or galena has nearly all the external properties of lead, and is heavier than several of the metals. Phosphuret of copper has a high degree of metallic lustre and hardness, and is susceptible of a fine polish. With these facts, there is no difficulty in the supposition, that, from the combination of hidrogen with metals, the general metallic properties may not undergo much change. Besides, hidrogen may be itself a metal, and these may be alloys.

No reason why hidrogen should destroy metallic properties,

if not itself a metal.

If these conclusions be just, it follows, that the metals which are the bases of the alkalis are still unknown to us in their pure state. The same conclusion follows perhaps with regard to the metallic bases of the earths. Nor is it impossible, that some of the common metals may contain hidrogen. These conclusions, it must only be remarked, rest on grounds altogether different from the hypothesis which has been proposed, that hidrogen is the common metallic principle, and have no connection with it. It is a conjecture supported at present by no species of proof. I do not suppose, that hidrogen is essential to the constitution of metals; but merely, that in common with other inflammables it is capable of combining with them; and according to this opinion there must exist in the alkaline metals, metallic bases independent of hidrogen.

Metallic bases of the alkalis yet unknown to us.

No proofs, that hidrogen is the general metallic principle.

The peculiarity which distinguishes the alkalis, regarding them as metallic oxides, and which undoubtedly presents an apparent anomaly—their great solubility in water, may be owing perhaps to the attraction which their bases have to hidrogen; for the affinities of a substance to a compound, to water for example, it is probable according to Berthollet's views

Solubility in water of the alkalis, considered as oxides.

views of affinity, are the modified affinities of that substance to the elements of the compound*. If the alkaline metals be admitted to exert an attraction both to hidrogen and oxigen, but more energetic, as their chemical relations evidently show, to the latter, it will follow, that the more powerful affinity will first be exerted, or the metal will combine with oxigen. But when this is diminished in force by the approach to saturation, the weaker affinity, that to hidrogen, will be capable of being exerted with equal effect—the attraction therefore of the oxidated base or alkali will now be exerted both to oxigen and hidrogen, in other words to water, with which it will combine. If the bases of barytes, strontites, and lime, be procured pure, they may be expected also to contain a portion of hidrogen, though perhaps to a less extent than the alkaline metals, as in their state of earths they are less soluble in water than the alkalis—indicating, according to this view, a less strong attraction exerted to hidrogen. There may thus be a series, through the other earths, to the common metallic oxides. And if this speculation can be admitted, we have the satisfaction of being able to point out the source of the peculiar properties, by which the alkalis, earths, and metallic oxides are distinguished as subordinate divisions of a class; and of farther showing, that the cause giving rise to these peculiarities is the same as that which is the cause of the equally striking differences by which they are distinguished in their simple or metallic form. The attraction of the real alkaline metals to hidrogen gives rise to their combination with it, and this is the cause of the levity, and probably also of the fusibility of potassium and sodium—the most striking properties by which they are distinguished from the common metals; and the same affinity to hidrogen is the cause of the solubility of these substances, when oxidated, in water, the property by which the alkalis and alkaline earths are separated as an order from the common metallic oxides. If this view be correct, it will also follow, that the metalloids obtained from the earths containing only a small por-

Source of the properties that distinguish the alkalis, earths, and metallic oxides.

* I have given at considerable length a view of this doctrine, which affords so many refined explanations of minute chemical phenomena, in the Notes to my System of Chemistry, vol. 1, p. 609.

tion

tion of hidrogen will be of intermediate specific gravity between the alkaline metalloids and the common metals, and that the earths themselves must have a degree of solubility in water, intermediate between the alkalis and the common metallic oxides.

I am, with much respect,

Yours &c.

Edinburgh, 21*st Feb.* 1811. J. MURRAY.

II.

Cheap and simple Construction of an Air Pump. In a letter from a Correspondent.

To Mr. NICHOLSON.

SIR,

AS you were so good as to publish a paper, which I sent to you some months ago, on a method of joining long or bent metallic tubes, &c., I have taken the liberty of troubling you with the following account of some attempts, which I have made, to simplify the construction of the air pump. Not being willing to go to the expense of one of the best air pumps, and at the same time equally unwilling to spoil my experiments by a bad one; no other alternative remained, than either to improve the old construction, or to invent a new one. How far this attempt may have succeeded, I must leave to the judgment of yourself, and of your readers.

Attempts to construct a cheap air-pump.

The following plan was the first that struck me: Let A B. (Pl. VII, fig. 1 and 2), represent the barrel of the pump; C C the piston rod, which is to be made solid, either of brass or steel, and accurately ground; and D the piston, which is made to move air tight in the barrel, by means of leathers as usual. The rod also moves in a collar of leathers, *a a*, and *b b*, is a cup to hold oil—so far the construction is the same as in Smeaton's air pump. The alteration which I have made is in the valves, which are of solid brass;

1st. construction.

c is a conical brass valve, well ground into the top of the barrel, on one side of the collar of leathers, and kept from being thrown out of its place, when open, by a pin and a small semicircle of brass. *d* is another brass valve, only this must be made much thinner than the other, that it may open with more ease by the condensation of the air below it. This valve has a long wire, *f f*, fixed to its under side; which wire, when the valve is shut, must project about $\frac{1}{8}$ of an inch below the piston. *e e*, *e e*, are two cross pieces, having a hole drilled through the middle of each, through which the wire *ff* passes. (One of these is represented separate at fig. 2, Pl. VIII.) When the valve *d*, is open, it is prevented from flying out of its place by a small button, at the end of the wire *ff*. *g g*, *g g*, are the passages by which the barrel communicates with the receiver. After the valves are finished, and fixed in their places, the top of the piston, and the under-side of the top of the barrel, are to be ground accurately to each other; so that when the piston is drawn to the top of the barrel, there may not be the least vacancy between them. The action of this pump will be as follows:

Action of this pump.

When the piston is moving upwards, the valve *d* will shut by its own weight, and by the condensation of the air above it, which having no other exit, will necessarily open the valve *c* in the top of the barrel; and when all the air is expelled, this last valve will also shut by its own weight; and as the top of the barrel and the piston are ground to fit each other, it is evident, that the *whole of the air*, above the latter, must be expelled. When the piston descends, the valve *c* (fig. 2.) will still continue shut, by its own weight, and *now* also by the pressure of the atmosphere, and the valve *d* will open, by the condensation of the air below it, and in the receiver; air from the latter, therefore, will rush through the passages *g g*, *g g*, into the upper part of the barrel, and will be expelled again when the piston is again drawn upwards; and this expulsion of a fresh barrel full of air, every time the piston ascends, will continue, till the air in the receiver is so far rarified, as not to be able to lift the metallic valve *d*. It is now that the use of the wire *ff* will be manifest, for by projecting below the piston, it is evident, that it must reach the bottom of the barrel, sooner than the

piston

piston does; and as the piston continues to descend, the valve *d* will be opened, by the pressure of the wire *f f* against the bottom of the barrel, and the air in the receiver will expand freely into the barrel, through the passages *g g*, *g g*, as represented in fig. 2. This air will also be expelled by the ascent of the piston; and this operation may be repeated as often as is thought necessary. As the valves will not be exposed to much friction, they will probably keep in order a long time; and if the pump is well made, there seems to be no impediment to its producing a perfect exhaustion*. But whatever care the workmen may take, it is probable, that a minute quantity of air will remain, and that in a state of considerable condensation (under the collar of leathers) between the piston-rod, and the top of the barrel; and also between the sides of the barrel, and the upper part of the piston: but this is a fault, to which every pump, I believe, is liable. It may also be objected, that, when the valve *d* is opened, by the pressure of the wire *f f*, against the bottom of the barrel, the air, by rushing suddenly out of the receiver, will occasion oscillations of the mercury in the gage: these oscillations, however, must be very trifling, not exceeding ¼ of an inch, as the valve *d* would open, were the pressure greater than that of ¼ of an inch of mercury. All these imperfections however, I hope, are completely obviated, by the following construction.—

At figs. 3 and 4, A B, as before, represent the barrel, C C the piston-rod, D the piston; *a a* the collar of leathers, *b b* the cup to hold oil, *c* the expelling valve of brass, *d* the valve in the piston of brass also, and *g g*, *g g* the passages 2d construction.

* It is obviously impossible, that the whole of the air in the receiver should be taken out in this way, so as to form a *perfect* exhaustion, provided we admit the workmanship carried to the highest degree of accuracy, so as not to have the slightest imperfection. Suppose the barrel and receiver to be of equal capacities. The first time the piston is drawn up, the air in the receiver will expand to double its former volume, or be of half the density it was before. After the second stroke of the piston, it will in the same manner be of half the density it was after the first. And thus the rarefaction will go on in a series decreasing in geometrical progression, which can never reach 0, as the last term must be half the value of that preceding. C.

by

by which the barrel communicates with the receiver. This pump differs from the former in the following particulars: first, in communicating with the receiver through the *top* of the barrel: secondly, in having the valve, in the piston, fastened to the end of the piston-rod: and thirdly, in the bottom of the piston being convex, and the bottom of the barrel concave. After the valve *c* and *d* are finished, and fixed in their places; the convex and concave surfaces of the piston and of the barrel are to be well fitted by grinding; so that, when they meet, all air may be excluded from between them. The reason for making the bottom of the piston convex, is, that the air may be prevented from lodging between the piston, and the sides of the barrel; as the leathers, by this construction, will reach the bottom of the barrel; for the bottom of the piston being convex, it will have no assignable thickness where it comes into contact with the sides of the barrel; as is shown at *k k*, Pl. VIII, fig. 3, the dark part representing the leathers. This pump will work as follows.—

Its action.

When the piston-rod (fig. 3) is pushed down, the valve *d* will shut by the pressure; and the air below it, having no other exit, will be forced out at the valve *c*, which will shut, by the reaction of the brass spring *l l*, as soon as all the air is expelled; and when the piston is again drawn upwards, it will continue shut, for the same reason, and also by the pressure of the atmosphere.—As the valve *d* is fixed to the end of the piston-rod, it will be impossible to draw up the piston, without first opening this valve; and by this means establishing a communication with the receiver, as represented fig. 4, by the same means also, the oscillations of the mercury in the gage will be prevented: during the ascent of the piston, therefore, the air will pass freely, through the passages *g g*, *g g*, into the lower part of the barrel; and when the piston is again pressed downwards, the first effect of the force applied will be to shut the valve *d*, and the air below it will be expelled at C as before.

I have had a pump made of this latter construction, and find it to answer my expectations, in every respect, as far as the principle is concerned; but I have never been able, entirely to prevent the different junctures of the pump (which

(which were secured with oiled leathers, in the usual manner,) from leaking: but, even with this imperfection, the mercury has frequently descended, in the siphon-gage, to within $\frac{1}{10}$, and once or twice to $\frac{1}{40}$ of an inch of the level. I have thought of a plan of obviating this imperfection, (to which, by the by, every pump is more or less liable) and shall be happy in communicating it to you, if it answers my expectations. There is one objection, which may be made to this pump; and that is, that the valve *d* will be exposed to considerable friction. This may be obviated, in some degree, however, by making it of iron, or steel, which is known to wear very little in brass; and still farther, by fixing it loosely to the end of the rod, in the same manner as the valves are fixed to the keys in Potter's patent flutes; or it may be entirely removed, at the expense of a trifling inaccuracy in the pump: as follows. C, (Pl. VIII, fig. 3,) is the piston-rod, D the piston, and *m* a brass plate, fixed to the end of the rod, instead of the valve *d*: to this plate a piece of leather is screwed, which is shaded darker in the figure; this leather, when the rod is pushed down, covers the hole *g g*, and prevents the return of the air into the upper part of the barrel. The hole may be made so small, as not to contain, in a barrel of 10 inches capacity, more than ·00001 of the air, independent of the size of the receiver. I have had a pump constructed with a valve of this description; and find it to answer perfectly well. It may be farther improved, by fixing a small wire, or valve, to the brass piece, (as is shown by the dotted lines) which will fill the hole *g g*, when the piston is pressed downwards: only care must be taken, that it do not project so far, as to open the valve *c* in the bottom of the barrel.

Its action

The price of a single-barrelled pump, of this description, (as represented, Pl. VIII, fig. 1,) made by Messrs. Dollond, will not exceed 10 guineas. It may also be made with two barrels. It is evident, that this pump will also act as a condenser, with a very little additional apparatus; which, as it will not differ from that usually employed, it is unnecessary to describe. I must beg leave to add, that the juncture, I formerly recommended, for bent tubes*, will be

* See vol. XXV, p. 284.

very

very useful here, if secured by oiled leathers, at each extremity of the connecting tube.

As the pump is entirely made of brass, and has no wood-work; it is very strong, and will no doubt last a long time.

I am, Sir,

Your constant reader,

L. O. C.

Query on centrifugal motion.

P. S. I beg leave to propose the following query, respecting professor Wood's new theory of the diurnal motion of the Earth. As the effect of the centrifugal force is, to cause a body to fly off from the circumference in the direction of the tangent; when any other force coincides with this, will it not be increased? and when any other force acts in a direction opposite to it, will it not be diminished? Accordingly, I have frequently observed, that the dirt, which flies off from the upper semicircle of a carriage-wheel, when moving rapidly, has a greater velocity, than that which flies off from the lower semicircle.

I am aware, that a part of this velocity may be accounted for in a different manner; but, I believe, the whole of it cannot.

III.

On the Interior of Plants. Letter I. *By Mrs.* Agnes Ibbetson.

To Mr. NICHOLSON.

SIR,

Students should avoid preconceived opinions.

WHEN first I began to dissect plants, something more than twelve years ago, that I might not involuntarily imbibe any system, or feel the least prejudice for any particular mode of thinking, but leave nature to write her fair history on my mind, and fix her various appearances in strong characters on my memory, I avoided with the greatest care all books of physiology; and it was not till many years after I had dissected and seen much of the course she pursues, and habituated myself to her forms and delineations, that I ventured

I ventured to see what others had thought and said on the subject, and compare the realities with their opinions. It was this mode of study, and no desire of contradicting others, that was the origin of these letters; my opinions were wholly formed by a view of the subject in nature, and not in books; as I have but too often found, that the eyes, if informed what they are to see, (if great care is not taken to prevent it) will be assisted by the imagination in seeing it. It has therefore been my constant endeavour to take the picture from my eye, and not my mind. Perfectly conscious also, that I have no pretensions to fine writing, (having neither time nor abilities for it) I must apologize to your readers for the homeliness of my style; endeavouring always to lay before them in the simplest language the exact truth —I pretend to no more.

The author's method of study.

How I proceeded to prepare myself for studying the present subject it is necessary I should show. Having collected most of my specimens in the autumn, that I might get them in various ages of bud, I chose the logs of different trees and shrubs, which I could select both in the nursery gardens adjoining, and in the gentlemens' grounds around; and having suspended them in a warm room to dry, I began in October to dissect, first stripping the rind, I drew an exact picture of the undulations and situation of the inner bark; then laying the wood completely bare, and exposing the various buds in their different ages working their way through the woody fibres, collected in parcels, some lowering to form a bed for the bud, while others, rounded over its head, seemed as a shield to defend it from all pressure. Imitating this with my pencil, as well as was in my power, I next divided the branches into two halves, very gently extricating the pith, and leaving the line of life alone pressing on the wood. Then are seen all the undulations this part makes to accompany each bud, substituting itself, whenever the pith is suspended, for the introduction of new wood, and marking by a cross line the situation of the fresh shoot. After drawing this also, with other wood of the same plant, I split it, and planed it very smooth. In this posture the stoppage of the pith is best seen; a matter of the greatest consequence to the plant. When this is drawn, the

the next and last step is the tearing off the limb, exactly as nature does it. Perhaps no appearance shows the formation of trees and shrubs, and explains their construction so well, as this way of examining them; it often points out the line of life drawn after the bud; shows many a bud creeping through its undulating prison; and above all exemplifies that curious construction, which I shall soon explain by the name of the minor obstruction. These studies, with a vegetable cutting taken from two or three different parts of the plant under investigation, formed the specimens from which I proposed judging of the interior of plants. I have already drawn 86 trees, and 123 shrubs, in this manner: but it is only the renewal of former labours, though the last four months have more particularly been dedicated to it. If I wished to recommend to my sex a complete cure for the vapours, I could not offer them a better than that, which, having employed me 13 hours out of the 24, never gave me a moment's weariness. How, indeed, is it possible to be tired of contemplating such wonders, and discovering such miracles! Enthusiasm here is the natural consequence of seeing.

The line of life.

The first thing necessary to the explanation of the dissection of the exterior of the stem of trees, shrubs, and smaller plants, is, that I should ascertain the existence of that important part, the line of life; which has by some means missed the observation of most physiologists. Du Hamel indeed (that excellent writer) observed, that all buds proceeded from it, and recommends it to the attention of future botanists. Linnæus (whose every hint is a trait of genius and science) finding that all buds, and all radicles, proceeded from this part, and not seeing the line of life, supposed it was from the pith they came; for the line of life is only to be distinguished by a pretty strong magnifier; but it is very easy to be discovered in all common vegetable cuttings, and still more easily traced from that line, which all philosophers have agreed to own of the first consequence in the plant, and which they call the impregnating vessels; but which is the identical line of life, which I have repeatedly taken out without breaking from the stem to the seed, and from the embryo back again to the stem. Its identity

will

will not therefore admit of a doubt; though I must say it would have appeared to me a greater miracle, that so important a part should do its office and then disappear, than that it should continue its consequence throughout the whole life of the plant. Besides, what would become of the buds formed in the trees? It is a fact easily ascertained, that no part can give life without being visited by this line; neither leaf nor leaf-bud, no part of the root, no part of the stem, but where this identical vessel is found: it runs up each bud to prepare it, as it does in the seeds, and the bud becomes capable of giving life; because it communicates the same string. Is it not proved, when a leaf-bud by an ignorant gardener has been mistaken for a flower-bud in budding? Du Hamel in vain endeavoured to make them grow; he bestowed the greatest care, but without effect; it was impossible, for they would not cement, not having that line, which first joins them together. Not so the radicle; possessing that line, which runs through it, if well managed, it will grow with ease. Those large buds also, which the roots of various plants throw up in the spring, such as the peony, &c., if the root did not send up the line of life, how would they continue their progress? like an excrescence they would decay and moulder away without bringing forth buds. To fix the absolute certainty of this, is of the utmost consequence to botany, particularly to physiology; I shall therefore collect all the reasons, which acted on my own belief, and persuaded me of this truth: viz. that the lines which run between the pith and the wood are really the lines of life, without which the vegetable cannot exist.

It is essential to the vitality of every part of a plant.

1st. It is the line from which all buds whatever take their rise. 2d. It is the line from which all radicles shoot. 3d. It is that line, which in grafting and budding is first seen to join. If the graft misses, this line turns black, and then no power can save the plant. 4th. If in budding it has not power or strength sufficient to join the new bud to the tree, it dies, and the wood makes no more efforts to unite. 5th. When in a frosty morning the apricots and peaches are hurt, and you wish to know whether they will recover; open a few flowers, and if the line of life is black, though the rest of the pistil may be perfectly free from the taint, despair of the

Proofs of its existence.

greatest part of your fruit: it never recovers after this, though the female may be some time before it decays. 6th. The flower bud will inoculate, but not the leaf bud. 7th. The line of life is never found in any leaf, but those which flower in the leaf; but here it is easily detected, meandering as in a flower stem. 8th. Take this line out of the bud when going to inoculate; it is not a quarter of the bud, but the bud grows not after such a dilapidation.—(It is the same line I took out of the bean.)—Every other part of the bud will renew itself, though badly; but this deprivation is sure to kill directly, while in so young a state. It is this line in potatoes, that enables the gardener to cut them into innumerable pieces, provided an eye is there, they will grow: What is an eye? a little knot of this string, which may be seen running from part to part. It is the same in all parting roots: in vain you would endeavour to make any other part grow, as I have often tried, but the smallest knot has the effect. So important is this line to every plant, that it is this which regulates the form of its colour, it is this, which, confining the pith, gives shape to the silver grain, or yearly circles; if this (as in the nerium oleander, or zelamicum,) runs into an irregular form, so also does the yearly shoot; but if in a regular manner it shapes its course, as in the dogwood, or still more exactly in the ephedra disticha, then that part of the plant follows its example with the most scrupulous nicety. The line of life generally consists of two or three rows of circular vessels, often very differently coloured from the rest of the plant, and generally yellow or a faint green in the spring. It has a very peculiar juice, which is thick and sweet, but often very bitter also; and quite different from the juices of any other part of the tree; and it becomes sweeter as it advances to the flower-branch, till it is found almost honey. In the cryptogamiæ and in grasses it is extremely conspicuous, as having (especially in the former) fewer lines to confuse the learner. What more can be said to prove its importance, except what relates to the bud? and to that I shall leave it; and turn to my next subject, mentioning that I include not in the present either water-plants, bulbous-roots, cryptogamiæ, or grasses.

Obstructions. There are three different sorts of obstructions in all plants. The

The 1st I shall call the grand obstruction, to distinguish it. It takes place only in two parts of the plant; that which separates the root from the stem; and that which divides the stem from the peduncle or flower stalk. In both cases the whole order and arrangement of the stem is so altered, that a pupil, accustomed to find six parts in regular gradation in its form, finds only three in the root of all the trees and shrubs. In the stem there are the rind, the bark, the inner bark, the wood, the line of life, and the pith: but in the root only, the rind, the wood, and the line of life. In some herbaceous plants, and annuals, a row of bark is found and some pith: this difference of the root does not take place immediately, but by gradual increase of rows, within the compass of an inch above ground; for they are the parts which have formed the seminal leaves, and the outward case of the embryo, that now grow up increasing in the bark and inner bark, till they arrive at the proper form of the stem; while the wood of the root decreases in like proportion: for the embryo growing up in no wise alters the situation of its parts. That outward case, which formed the holders to the seed, and the cotyledons, again, when out of the seed, forms the bark and inner bark of the stem, returning to the part from which it came; so that nothing can equal the beautiful simplicity of nature's designs, if we did not ourselves load them with difficulties. The root is certainly a reservoir for nourishment, which the radicles bring in quantities, which is contained in the wood, and which it dispenses to the tree by the means of the stem. The radicle is formed of the same parts as the root, having the line of life in the middle, ready to dispense to any minor radicle.

in plants. 1st grand obstruction, separating the root from the stem.

The next grand obstruction is that of the peduncle; dividing the flower stem from the branch. Linnæus alone hinted at this; and so plain is it to be seen, that to look is to be convinced. Cut a slice from both, and compare them; and their arrangements will be found wholly different. On examining the wood, it will appear to divide into various little parcels, answering to the number of stamens the flower contains; while the inner bark separates into the number of petals; and the line of life, fixing in the middle, generally denotes the figure and divisions of the pericarp. I have

2d. grand obstruction dividing the flower stem from the branch.

I have seldom seen this vary; nay, even in compound flowers of the syngenesian order, where the divisions are so often repealed, and the compartments of each perfect flower so very minute, the pin-holes of the stamen surround the larger apertures of the pistil, and, accompanied by the vessels appropriated to each petal, are as exact and as exactly delineated as below the largest flower, a campanula, or a lilly. Let any person see a vegetable cutting of the peduncle of a syngenesian superflua in a solar microscope, and he will have no doubts. This is made still plainer by the bursting of the vessels of a double flower, and the confusion that ensues, one part growing for another, one on another; sometimes the wood escapes bursting, and then the males are perfect; sometimes the line of life does its office till it passes a second time into the pericarp, in the way to the seeds, and then fails; and in this case all is perfect but the seeds, which do not vegetate. In my opinion, it would be a much greater wonder to suppose such different parts to be formed of the same matter, than to conceive that each had its appropriate juices. But I mean not this as an argument. I never allow myself to reason in this manner. I rely on experiment, observation, and dissection only, and I may say, without vanity, on a labour, few would submit to; and none can appreciate but those who know me.

Middle obstruction. Stoppage of the pith.

Having described the grand obstruction, I shall now show the middle one. This is formed by the stoppage of the pith. The pith is most properly esteemed a source of moisture to the tree; but when I come to show the manner of passing of the buds, I shall prove a still more direct use for its moisture. The pith stops in all trees at every new branch, forming a piece of wood, which generally fills up nearly half an inch, apparently to give the plant, by a great accession, strength sufficient to invigorate and produce healthy branches. The pith therefore recoils, not only in the new bud, but in the main stem also. But in smaller plants the new shoot alone generates new wood, which appears an accession sufficient for such slender twigs; where the pith occupies so large a portion of the plant: and in some annuals and herbaceous plants the pith stops not at all, but is contented with drawing the line of the corners of the wood closer;

closer, sometimes adding a small piece to the interior, for the purpose of collecting nourishment. There is, however, a sort of hollow plant, which forms a complete stoppage; but it generally grows in wet ground; and will be noticed with water plants, in another letter. These various differences very exactly regulate many important points belonging to the construction of the interior of the stem, viz. the quantity of new formed wood; the situation, manner of passing, and regulation of the buds; and particularly the formation of the minor obstruction, which I shall now explain.

Minor obstruction. Imperfect stoppage of the pith.

This last seems more particularly to belong to those plants, which, without stopping the pith, a little impede its progress; and the curious construction of which is such, that, had I not already shown it in the French bean, I should have deferred mentioning it here, till better able to comprehend its structure. I have found the marks which indicate the aperture in the largest trees. In the Weymouth pine, and in the silver fir, it seldom fails to give proofs of its excentricity, even at the exterior of the tree; but I never saw the ball, except in those which are running plants, or at least shaped like them; that is, that have the large shooting branches made like the shoulder of a man, with a ball and socket*. I at first conceived, that in these small plants the branches were to turn on the ball; but I have found many specimens, where the limb was too firmly fix'd for this purpose. I then conceived it the foundation for tendrils; but I have found it where no tendrils are: besides, it is too diminutive. Not able to dedicate more than one figure to this service, I must delay many extraordinary proofs of its existence; and many curious peculiarities of its structure, on account of the buds, which demand all the sketches I can venture to give. I shall only say therefore, that it is fixed in the under part of the branch, where no bud ever comes, but is defended with the greatest care.—(See Pl. IX, fig. 1.) †

* I understand that I have been thought to commit myself in comparing a part of the seed to the breast of a bitch, having disapproved of animal comparisons; but it was a long analogy I found fault with, not a simple likeness in form, and surely there is a great difference between such a one, and an analogy reaching to perspiration, circulation, and even perception

† This, with some other figures belonging to the present paper, is ob-

In

In my next, the reader shall know as much as I am acquainted with, and that is but little.

Annual formation of wood.

In all trees and shrubs, one row of new wood is formed each year; but this is not all, some new wood is always generated to accompany each bud. Small plants, however, the pith of which is little, if at all separated; and where the line of life runs from the middle of the pith in various vessels to the exterior, all form two, if not three rows of wood each year. Cabbage, and its offspring, also turnips, carrots, &c., show one for every time of flowering, whether dilapidation takes place in the part above, as in the cabbage and cauliflower, &c., or not. The same accession of new wood takes place in the root also. There are many very extraordinary points of construction in these small plants; such as the bastard vessels running often through the pith, and across to the wood at the other side; and innumerable other things, which, belonging rather to peculiar plants, than to nature in general, I shall therefore leave to the next letter, and turn to my last and chief subject.

Buds pass through the wood.

On this subject, it is quite astonishing to me that any one should differ, since the evidence is so exact and plain. If we look at the floor of a room, a piece of wood, a tree, &c., they should all together or separately teach us, that the greatest part of the buds must pass through the wood; since there is no wood that is not marked with them to the edge of the pith, that is to the line of life. In one of my former letters, I gave a long account of the formation of the leaf-bud, mentioning how much it differed from the flower, or leaf and

Formation of the leaf-bud,

flower-buds. Formed almost wholly of the bark and inner bark, and placed there, it is the lengthening of the threads of the bark, which weave the leaves; and all the time they are forming, the bud remains stationary; nor do they draw any assistance from within, except a little wood in the shape of nourishing vessels, which surrounds the spiral wire. But

and of the flower-bud.

very different is the formation of the flower-bud. Botanists have perceived this difference, and yet appear to derive them both from the same source. Most physiologists have considered the bud as proceeding from that row of new wood,

bliged to be deferred to the supplement, which will be published the 1st of next month.

which

which is formed each year next the bark. If so, how comes the bud into the farther part of the wood? The destination of the bud is to spring without; and to this part it will of course proceed by the shortest road, so that, if it began at the albumen, it would soon attain its situation. But the flower bud, and leaf and flower bud, are really formed on the line of life. When a bud is going to shoot, the line of life forms a knot, and a quantity of albumen is directly generated round it; and the knot covered by a scale. While this is doing, the wood is preparing for its reception. Some parcels of the old wood, sinking before it, form a bed; and others, rising above it, prepare a sort of arch over it, in the middle of which it passes, as in a vaulted cave without hindrance, or pressure. I have followed hundreds with a sharp knife; and if this is done without injuring the under part, it will be perceived to shine below where the bud is to go, as if varnished. Probably the line of life brings with it a softening liquid for this purpose; and this line is placed next the pith, that the latter might afford the bud plenty of moisture to support and invigorate it in its first shooting, and during its journey to the exterior of the tree. Besides, I should suppose that moisture would much contribute to bending the vessels of the wood into the shape required; basket-makers moiſten continually their twigs, to bring them to the necessary form; and nature in this respect acts as the basket-maker. The best sort of wood for seeing the progress of the bud is a pollard, cut for the second time, and then taken off about February. The buds then are springing from the line of life in numbers. I have a beautiful specimen of a willow, where, from the quantity of buds egressing, the wood forms a complete scollop the whole way. (See Pl. IX, fig. 2.) It is probable, that these may be the buds of the summer; though I have many reasons to believe that a bud is not near so long growing as is generally supposed. When the bud has arrived at its destination in the bark, it stops; the leaf and flower bud to form its leaves, which it does before the flowers; and both to gain those scales and clothing, that enable them to bear the outward air; and, if the air is not mild enough, they remain till it is fit. Every accession of wood at the exterior, forms a sort

Best seen in a pollard.

sort of cradle for buds, where often two or three are found resting, and completing their scales, and fiting themselves for their entrance into the world. It is this situation, that has been taken for their birth; but it is only their place of repose after their long journey. In young wood there is so much pith, that they have but little way to travel; but in old wood it is very curious to behold the length of way through which they will go, the wood-vessels arranging themselves in the most exact order, to let them pass. In the yew, where there are so many buds, they will continually derange even the yearly or silver grain, and oblige this also to prepare them a proper and hollow passage. In all plants where the pith stops not, the line of life runs in the middle of the pith, constantly making its way to the exterior, with the buds. I have in some annuals, and some herbaceous plants, good specimens, where the buds are passing in different directions. I shall give one of the malvas (See fig. 5).

Hard mortar penetrated by a vine-bud, and stone by a fig-tree.

At first it appears impossible, that so soft a substance as a bud should make its way through such a quantity of hard matter; but many instances have been known, where vine-buds have passed through the hardest mortar; and I once saw the branch of a fig-tree, which had made its way through Bath stone. But in the present case it is not the bud, which forces forward: it is the wood, which in a far more extraordinary way prepares for its reception, by the preceding of even a softer substance than itself, albumen. Without absolutely saying so, we are very apt to set bounds to the power of God; and circumscribe it within our capabilities; yet every day's study should convince us, that it is only little by little, that we can even comprehend the glorious works of the Creator; and the astonishing power he has shown in this beauteous world he has formed.

The bud will not permit any thing in the nature of wood to turn it from its course, which is always the shortest line that can be drawn from almost the centre to the circumference. I once saw a horse-shoe, which had by some means got into the wood; the buds in vain essayed to pierce it, they all died in the act of trying; some of them made an effort (being stronger) to pass round it; but died also before they could reach their destination: at last the line of life

life began to form with a new pith, and the part, where the shoe was, decayed ; but in this state the tree was cut down, or it would have produced a still more instructive history of the force of buds ; which had for three years contested the point continually with this hard substance.

All that farther concerns this subject is more peculiar than general; I shall therefore defer it to my second letter. I was anxious indeed to clear my way, that, having laid the foundation, I might proceed to the many wonderful parts, which I shall hope to analyze in my next.

I am, sir,

your oblig'd humble servant,

AGNES IBBETSON.

Explanation of the Plates.

Pl. VIII, fig. 4. The bud extremely magnified, the wood passing over and under.

Fig. 5. Section of the malva, showing the bud on the line of life (*k*) the two buds : (*kk*) the albumen in the herbaceous plants, but it is seldom so regular in form.

When the bud is to be formed, the line of life makes a knot ; this knot is directly covered with a scale, and both within and without with the albumen, which grows ; and I do not think there is any other increase of the bud, till it arrives at its cradle in the bark.

It may be thought, that there are repetitions of my former letters, but to describe the outward part of plants, and afterwards the inward, it is scarcely possible to avoid repetitions, anxious as I am to be perfectly understood as I proceed.

I have given also a specimen of a vegetable cutting of the cactus, (fig. 6.) to show the curious manner in which the buds follow each other, and are discovered on the line of life. *ll* the bud.

Pl. IX, fig. 1. The appearance of the minor obstruction, and the sort of plants that more peculiarly show the ball, exhibited in scrophularia. *a* the ball, *bb* the bud, or, when it is wanting, the cradle where it reposes, *c* the hollow of the pith.

Fig. 2. Appearance of the buds in a willow.

Fig. 3.

Fig. 3. A piece of wood torn off the branch, showing the minor obstruction just below the pith.

Fig. 4. A piece of wood cut lengthwise, with several buds, *dd*, travelling from the line of life to the exterior, through the wood; *ee*, buds travelling through in another direction; *ff*, the new wood, or albumen.

IV.

Observations on the Oxides of Iron, with a Discussion of their Nature. By Mr. J. H. HASSENFRATZ.

(Concluded from p. 193.)

FIFTH METHOD. *Oxidation of Iron by acids.*

Oxidation of iron by acids.

WE know that iron is oxided when dissolved in acids, 1st, from the gas that is evolved; and 2dly, from the nature and state of the precipitates after the solution. We will examine, by each of these two methods separately, what proportion of oxigen combines with the iron, to promote its solution.

Of the quantity of oxigen deduced from that of the gas evolved during the solution.

Calculation of the oxigen from the gas evolved.

According to the nature of the acids the gasses collected will be different. When the iron is dissolved in nitric acid, the acid itself is decomposed, its oxigen unites with the metal, and the nitrous gas, that was combined with it, is evolved. In concentrated sulphuric acid too, the acid is decomposed, and sulphurous acid is disengaged: but in sulphuric acid diluted with water, muriatic acid, acetic acid, &c., it is the water that is decomposed, and hidrogen gas is obtained.

Lavoisier with nitric acid.

Lavoisier dissolved iron in nitric acid, collecting and measuring the nitrous gas evolved *. Having ascertained by previous experiments the quantity of oxigen, with which this gas must be combined to form nitric acid, he thence deduced the quantity combined with the iron during its solution. He found, that the results were very variable; and that

* Mém. de l'Ac. des Sc. 1782, p. 492 and following.

that he obtained more or less gas, according to the temperature at which the experiment was made. From some experiments he inferred, that the proportion of oxigen combined with 100 parts of iron must be 32. In one experiment, made between 25° and 30° of R. [88° and 100° F.], 32·27 of oxigen combined *with* 100 of iron *. Notwithstanding these differences, he conceived the quantity of oxigen combined with iron dissolved in nitric acid could be carried only to 29 per cent of iron.

It is difficult to estimate by this method the proportion of oxigen in a constant state of the oxide, because, 1st, the proportions of gas obtained vary with the temperature and the degree of concentration of the acid; 2dly, it is extremely difficult to obtain a uniform nitrous gas; 3dly, the iron is always oxided in different degrees. Objections to this method.

Bergman †, having dissolved separately more than forty varieties of iron in sulphuric acid diluted with water, and in muriatic acid, obtained with the former from 36 to 52 Swedish cubic inches of hidrogen from the docimastic centner of iron, and with the latter from 15 to 51 inches. Bergman with sulphuric and muriatic acids.

Guyton quotes an experiment of Priestley, in which the English philosopher obtained 155·5 cubic inches of hidrogen gas with 27·55 [grs.] of iron; which would carry the quantity of oxigen combined as far as 39·7 parts to 100 of iron ||, Priestley.

Lavoisier, having dissolved 100 grs. of iron in dilute sulphuric acid, obtained 110 cubic inches of hidrogen gas, which would make the quantity of oxigen combined with 100 of iron 27·3. Lavoisier.

Vandermonde, Berthollet, and Monge, dissolved 21 varieties of iron in sulphuric acid diluted with water. From the purest iron they obtained 78 ounce measures of hidrogen gas, at the temperature of 12° R. [59° F.], and pressure Vandermonde, Berthollet, and Monge.

* Ib. p. 497. † Analysis of Iron.

|| Not being able to turn to the passage in Priestley, as the reference is to the Encyclopédie, I cannot verify the numbers here given. Mr. Hassenfratz appears to have taken the numbers as representing French cubic inches, and French grains; but supposing them to be English inches and grains, which is very probably the case, the proportions will no give more then 27·69 oxigen to 100 iron. C.

28 in.

28 in. [29·8], which would make the quantity of oxigen combined with 100 of iron 29*.

Vauquelin.

Vauquelin, having dissolved several kinds of steel in dilute sulphuric acid, obtained from 108 to 121 cubic inches of hidrogen gas; whence the quantity of oxigen combined with 100 of iron would be from 26·6 to 29·9†.

Experiments to be excluded.

From the products of hidrogen gas obtained by dissolving iron in dilute sulphuric acid we must exclude those of Bergman, which exhibit too great differences to deserve any confidence; and that of Priestley, which appears much too considerable not to induce the suspicion of some errour, or that great part of the iron was oxided at a maximum‡.

Conclusion from the others.

The other experiments lead to the conclusion, that the quantity of oxigen combined with iron varies between 26·6 and 29·9 to 100 of the metal dissolved.

But iron is seldom pure

Iron is seldom pure, always containing carbon, scoriæ, and sometimes oxidule. The iron that gave Vauquelin 121 cubic inches of hidrogen contained 0·00633 of carbon, and 0·00626 of siliceous earth.

so that we may estimate the oxigen at the highest.

The impurity of iron leads to the presumption, that the quantities of hidrogen obtained were less than would have been produced, if the metal had been perfectly pure; whence we may consider the greatest quantity of hidrogen obtained as resulting from the purest iron: and that analysed by Vauquelin contained at least 0·01 of foreign matter, so that we may carry the proportion of oxigen combined with 100 of iron dissolved in dilute sulphuric acid as far as 30 without danger. But, according to Thenard, the iron in this solution is in the state of white oxide; whence the white oxide must contain 30 of oxigen to 100 of metal, or consist of 77 parts iron and 23 oxigen.

Thenard's white oxide.

Of the quantity of oxigen combined with iron ascertained by the oxides contained in the solutions.

Calculation of the quantity

The quantity of oxigen contained in the oxides separated from their solutions may be ascertained in three different

* Mém. de l' Ac. des Sciences, 1786, p. 166 and following.

† Journ. des Mines, No. 25, p. 22.

‡ If my conjecture, expressed in a preceding note, be right, this remark on Priestley's experiment is unnecessary. C.

modes:

modes: 1st, by distilling the solution to dryness, washing the precipitate, and drying; 2ndly, by precipitating the oxide by an alkali; 3dly, by precipitating in the metallic state, by means of iron, the dissolved oxides of other metals, the quantity of oxigen contained in which is already known.

in the oxides themselves.

1st *Method*. Lavoisier dissolved iron in nitric acid, evaporated to dryness, and obtained a red oxide, the weight of which was from 40 to 50 per cent more than that of the iron dissolved.

Red oxide with nitric acid by Lavoisier;

Bucholz dissolved iron in diluted nitric acid, evaporated it to dryness, and kept it in a red heat for a quarter of an hour. In three successive experiments the increase was 42 per cent.

Bucholz,

Gueniveau, mine engineer, writes to me from Paris, on the 30th of September, 1807: "We also have been employed on the same subject (the oxidation of iron by the nitric acid), and we conclude, that the oxide is composed of 100 metal with 44 oxigen. We employed the same method as Mr. Bucholz." This experiment appears to have been repeated in the laboratory of the council of mines at Paris.

and Gueniveau.

Iron dissolved by nitric acid is commonly in the state of red oxide. Lavoisier says, that, having calcined this oxide to reduce it to the state of ethiops, it lost so much of its weight, as to retain no more than 25 or 30 of oxigen to 100 of iron *.

Part of the oxigen said to be expelled by heat by Lavoisier.

It is difficult to place confidence in this result of Lavoisier: 1st, because there is no indication of his having taken the necessary precautions, to oxide completely the metal he dissolved: 2dly, because, according to the experiments of Berthollet and Fourcroy, the oxide does not part with any of its oxigen to return to the state of oxidule, to whatever temperature it be exposed.

but this questionable.

It remains for us therefore to compare the two results of Bucholz and Gueniveau. As it is difficult for us to know with certainty, whether the oxide of Bucholz did not still contain some portion of oxidule mixed with it; and whether the iron, which he employed, were perfectly pure; every thing leads us to prefer the second result, which gives the larger proportion of oxigen. Both however having taken the

Comparison of the other results.

* Mém. de l'Ac. des Sciences, 1782, p. 5.

same

same precautions, and employed the same care in their experiments, we will leave it for the present undecided, whether 100 of iron be mixed with 42 or 44 of oxigen in the red oxide.

The oxide precipitated by an alkali, by Guyton,

2nd *Method.* Guyton dissolved iron in nitric acid, and precipitated it with salt of tartar. The precipitate, after being repeatedly washed and dried, had increased 32·1 on 100 of iron.

and Lavoisier.

Lavoisier precipitated the nitric solution of iron by caustic alkali, and obtained two different precipitates. 1st, When the solution was weak, made without heat, and precipitated by pure caustic ammonia, this illustrious chemist had a black precipitate, in which the weight of oxigen to 100 of iron was 30. 2dly, When the acid was concentrated, and the solution promoted by heat, the iron received an increase of 40 or 50 per cent, whatever were the acid employed; and this precipitate, after being calcined, retained only 25 or 30 parts of oxigen to the 100 of iron.

Oxide of iron in sulphuric acid diluted with water, when precipitated from its solution, and afterward dried in close vessels, Lavoisier found to be in the state of ethiops, and containing from 25 to 30 parts of oxigen to 100 of metal.

Probable source of error in Lavoisier's exp.

This great diminution of the weight of the precipitated oxides obtained by Lavoisier might lead us to suppose, that some coally substances had got into the vessel containing the oxide, and contributed to the reduction of part of the oxide to the metallic state.

These experiments inconclusive.

It is difficult to draw accurate conclusions from these experiments, because it is not certain, whether, by virtue of that chemical action of which Berthollet has proved the existence, a portion of the alkali had not been carried down by the precipitate, and a portion of the oxide retained by the menstruum. In fact Fourcroy found, that the oxides of iron precipitated by ammonia retained a portion of the precipitant.

Solution of silver precipitated by iron by Bergman.

3d *Method.* Bergman remarked, that 20 parts of steel of Osterby threw down 66 parts of silver dissolved in sulphuric acid; whence it follows, that 100 parts of silver would have been precipitated by 29 iron. From this Lavoisier concludes,

cludes, that 100 parts of iron would take up 37·29 of oxigen in this e xeriment.

The result here depends on too many circumstances, to make known the proportion of iron contained in the oxide or oxidule. In fact the quantity of precipitate compared with the iron dissolved depends, 1st, on the quantity of oxigen combined with the silver in the solution; 2dly, on the proportion of silver precipitated from nitric acid by mercury; 3dly, on the quantity of oxigen which mercury takes up on being dissolved in nitric acid: and if these three data be not strictly accurate, the final result cannot be determined with precision. Proust says he has discovered, that silver may be oxided either at a maximum or a minimum in nitrate of silver*; and hence we have a fresh source of errour in the inference of Lavoisier.

Objections.

Two nitrates of silver.

Bucholz attempted to determine the proportion of oxigen in the oxidule of iron, by supposing that in the oxide to be known. With this view he passed the vapour of water over red hot iron, and obtained an oxide, which on trituration furnished a black powder. This, being subjected to the action of muriatic acid, and to several other tests, was found to be an oxidule of iron.

Bucholz formed the protoxide of iron by means of heat and water,

This oxidule was dissolved in nitric acid in the same manner as iron, the fluid evaporated, and the residuum kept at a red heat for a quarter of an hour, when it had gained an addition of one tenth to its weight. This, on the supposition that the red oxide contains 42 of oxigen to 100 of iron, would give 30† of oxigen to 100 of iron for the oxidule; and on the supposition of the red oxide containing 44, the oxidule would contain 31 to 100 parts of metal; and 100 of oxide [oxidule] would be composed of 76·3 iron, and 23·7 oxigen, which agrees with the results of Lavoisier and Bucholz.

and converted it into peroxide by nitric acid.

Calculations from this.

6th method. *Of the quantity of oxigen determined by the decomposition of nitre.*

Guyton projected equal parts of iron filings and nitre into a red hot crucible, washed the residuum repeatedly in boiling water, and obtained an oxide, in which the iron had received an addition of 34 parts to 100.

Iron oxided by nitre, by Guyton,

* Journal de Phys 1799, vol II, p 221.
† 29 1 only. C.

Lavoisier

and Lavoisier, Lavoisier imagined, that the iron employed by Guyton was not pure, because, in the experiments he made, he obtained an increase of 45 to 100.

differed greatly. The difference between the results of Guyton and Lavoisier is too great to allow us to deduce from these experiments the proportions of oxigen contained in the oxide.

Conclusion from the experiments recited respecting the proportions of oxigen and iron contained in the different oxides.

General conclusions. The numerous experiments on the oxidation of iron, that have been mentioned, and which were made by persons celebrated equally for their talents, sagacity, and precision, may be divided into two classes. The first, executed at a time when the science was not sufficiently advanced, to indicate all the causes that might influence the results, are not sufficiently accurate for us to make use of them. The second, made with the care and precision, that the present state of the science admits, ought seemingly to enable us to determine the proportions of oxigen and pure metal in the oxides, if not accurately, at least with so near an approximation to the truth, that we may employ them with advantage in those chemical analyses in which iron is present.

Red oxide. The proportion of oxigen in the red oxide of iron exhibits some uncertainty. It was found to be 42 to 100 of iron by Bucholz; 44 by Mr. Gueniveau; and 45 by Desroches and myself.

Several considerations lead me to adopt the proportion of 45 of oxigen to 100 of iron, supposing the three experiments, which give these different proportions, to have been made with the same degree of accuracy; according to which the composition of 100 parts of red oxide would be in round numbers 69 iron, and 31 oxigen.

69 iron to 31 oxigen.

Reasons for taking this proportion. Among the considerations that lead to the adoption of this proportion, the following may be distinguished. 1st, The method of oxidation by the air assisted by heat, which is the most direct and simple. 2d, The difference between the results of Bucholz and Gueniveau, 44 being nearer to 45 than to 42. 3d, The difference in specimens of iron. Iron rendered impure, either by the union of carbon, by oxidule remaining in it, or by scoriæ, would receive a smaller addition

addition of weight from the combinaton of oxigen, than iron in a purer state. Now as Bucholz did not analyse his iron separately, it is difficult to decide on his account. In the analysis there is an element very difficult to be determined in the present state of our knowledge, this is, the quantity of oxidule remaining. Now as, from these considerations, we should regard the highest result as nearest approaching the truth, this leads to the adoption of 45.

The proportion of oxigen in the black oxide appears to vary between 30 and 31·8 of oxigen to 100 of iron. It is 30 in reducing the black oxides of the valley of Aost: it is 40 in reducing the black oxide of the isle of Elba with hidrogen gas. On dissolving the black oxide in nitric acid, the oxidule increases 10 per cent in passing to the state of red oxide. From this experiment it is inferred, that the black oxide contains 30 of oxigen to 100 of iron, supposing the red oxide to contain but 42 of oxigen. If, however, this oxide be estimated at 45, agreeably to our experiments, the black oxide will contain 31·8 to 100 of iron. Lastly, it amounted to 31·38 in the experiment where Lavoisier oxided iron by exposing it red-hot to the vapour of water in a copper tube; and to 30·38 according to Bucholz. Black oxide.

There appears some difficulty in choosing between the results of 30 and 31·8 of oxigen to 100 of iron in the oxidules, yet we think the higher ought to be taken, because, 1st, the proportion derived from the oxidulated ores of the valley of Aost is liable to various uncertainties, respecting the identity of the mineral fused and the mineral analysed: 2dly, carbon always combines with the regulus, and it is difficult to determine its quantity precisely: 3dly, it seems more proper to calculate the difference of proportion between the black oxide and the red, by taking the latter at 45 than at 42: 4thly, of all the experiments, in which the black oxide has been obtained, that where the vapour of water was employed is the most direct: 5thly, considering the impurity of iron, the highest term of oxidation is always to be preferred, when its state is well determined. Reasons for taking the higher estimation,

From all these considerations we shall presume, that the black oxide is composed of 31·8 oxigen to 100 of iron; or 76 iron to 24 oxigen.

 whence

whence it will follow, that 100 parts of black oxide of iron are composed in round numbers of 76 iron and 24 oxigen.

White oxide. No method of obtaining the white oxide of iron is yet known, but that of dissolving the metal in an acid. When the oxide thus obtained is precipitated, it immediately combines with oxigen to generate black oxide, and frequently red oxide; so that we have no method of finding the proportion of oxigen in white oxide but by calculating from the hidrogen gas evolved during the solution.

In this way we found the proportion of oxigen to vary between 26·6 and 30. To choose between these two numbers, taken from the experiments of Vauquelin, we should know the state of the iron dissolved, and the mode in which the solution was conducted. Calculating from the different degrees of impurity in iron, we shall be led to adopt the proportion of 30; yet, I think, it would be preferable to take that of 29 of oxigen to 100 of iron, deduced from the experiments of the French academicians, Monge, Vandermonde, and Berthollet: because, 1st, it was the highest product they obtained from 21 varieties of good iron, which they dissolved: 2dly, they were particularly careful in measuring the gasses they obtained; 3dly, they reduced these measures to a uniform temperature and pressure, whence it was easy to calculate the exáct quantity of oxigen corresponding; 4thly, in the experiment of Vauquelin, which may be considered as very accurate, the omission of the temperature and pressure, at which the gasses were obtained, renders us unable to compute exactly the quantity of oxigen.

77·5 iron to 22·5 oxigen. From these considerations it follows, that the white oxide is composed of very nearly 29 parts of oxigen to 100 of iron; and that 100 parts of white oxide contain 77·5 iron and 22·5 oxigen.

Addition.

Difference between the native oxides of Aost and Elba. Mineralogists know, that there are very characteristic differences between the oxidule of iron of the valley of Aost, which the celebrated Haüy designates under the name of oxidulated iron, and the oxidule of the isle of Elba, which he calls oligist iron. The primitive form of the first is the octaedron, and it yields a black powder; the primitive form of the second is a rhomboid, somewhat acute, and it yields a red powder.

These two oxidules appear to differ in the former being only

only a pure oxidule, similar to that obtained from the oxidation of iron by water, the decomposition of which is assisted by heat, and consisting of 0·76 iron to 0·24 of oxigen; and the latter being a compound of red oxide and black oxidule of iron, in which the proportions of these two substances are variable.

The powders of oxidulated iron, and of oxidule of iron obtained by heat and water, appear to be identical. We have analysed several specimens of ore from the island of Elba, and found in every one very variable proportions of the two oxides. Of the two varieties mentioned in the preceding observations, one contained 33, the other 40 of oxigen to 100 of iron. Some others, that we analysed both in the moist and dry way, contained 35, 36, and 38 of oxigen to 100 of iron. One alone yielded us only 30, like the ore of the valley of Aost: but we had reduced and analysed separately two fragments taken from different parts of the same specimen.

Among the fine crystals of the isle of Elba a great deal of red oxide of iron is found; and it is principally in this state, that the ore is extracted for the smelting houses. In the mines of the valley of Aost, on the contrary, the oxide is much more rare, and the ore is pretty commonly extracted for the works in the state of oxidule.

This addition I give only as a sketch, that may assist the mineralogist in his researches, and facilitate the discovery of the causes of the differences presented by these two species of iron ore, which the learned Haüy had already divined with that sagacity, which he employs in all his labours. These merely hints to mineralogists.

V.

On the Parts of Trees primarly impaired by Age. In a Letter from T. A. KNIGHT, *Esq. F. R. S. to the Rt. Hon. Sir* JOSEPH BANKS, *Bart. K. B. P. R. S.**

MY DEAR SIR,

IN the first communication I had the honour to address to you, (it was in the year 1795,) I stated the result of many experiments

Grafts last but for a limited period.

* Phil Trans. for 1810, p. 178.

experiments on grafted trees, from which I inferred, that each variety can be propagated with success during a limited period only; and that the graft, or other detached part of an old tree, or old variety, can never form that, which can with propriety be called a young tree.

What organ first fails?

I have subsequently endeavoured to ascertain which, among the various organs that compose a tree, first fails to execute its office, and thus tends to bring on the incurable debility of old age; and the result of the experiments appears sufficiently interesting, to induce me to communicate an account of them.

Analogy between vegetable and animal organs.

Whatever difference exists between the functions of animal and vegetable life, there is a very obvious analogy between some of the organs of plants, and those of animals; and it does not appear very improbable, that the correspondent organ, in each, may first fail to execute its office; and satisfactory evidence of the imperfect action of any particular organ can much more easily be obtained in the vegetable than in the animal world. For a tree may be composed, by the art of the grafter, of the detached parts of many others; and the defective, or efficient, operation, of each organ, may thus be observed with the greatest accuracy. But such observations cannot be made upon animals; because the operations necessary cannot be performed; and therefore, though there would be much danger of errour in incautiously transferring the phenomena of one class of organized beings to another, I conceive, that experiments on plants may be, in some cases, useful to the investigator of the animal economy. They may direct him in his pursuits, and possibly facilitate his inquiries into the immediate causes of the decay of animal strength and life; and on a subject of so much importance to mankind, no source of information should remain unexplored, and no lights, however feeble, be disregarded.

Experiments may be made on trees that cannot on animals.

Organs of vegetables and animals compared.

Naturalists, both of ancient and modern times, have considered the structure of plants as an inversion of that of animals; and have compared the roots to the intestines, and the leaves to the lungs, of animals; and the analogy between the vegetable sap and animal blood is very close and obvious. The experiments also, of which I have at

different

different periods communicated accounts to you, supported by the facts previously ascertained by other naturalists, scarcely leave any reasonable grounds of doubt, that the sap of trees circulates, as far as is apparently necessary to, or consistent with their state of existence and growth.

No deficiency in the roots occasions the decay of trees.

The roots of trees, particularly those in coppices, which are felled at stated periods, continue so long to produce, and feed, a succession of branches, that no experiments were wanted to satisfy me, that it is not any defective action of the root, which occasions the debility and diseases of old varieties of the apple and pear tree; and indeed experience every where shows, that a young seedling stock does not give the character of youth to the inserted bud or graft. I, however, procured plants from cuttings of some very old varieties of the apple, which readily emit roots; and these plants at the end of two years were grafted, about two inches above the ground, with a new and very luxuriant variety of the same species. These grafts grew very freely, and the roots themselves, at the end of four or five years, probably contained at least ten times as much alburnum, as they would have contained, had the trees remained ungrafted. The roots were also free from any appearance of disease, or defect.

Plants from cuttings of old varieties of the apple grafted.

Some crab-stocks were at the same time grafted with the golden pippin, in a soil where the wood of that variety rarely lived more than two years: and I again grafted the annual shoots of the golden pippin with cuttings of a young and healthy crab-tree, so as to include a portion of the wood of the golden pippin between the roots and branches of the native uncultivated species, or crab tree; and in this situation it grew just as well as the wood of the stock and branches. Some branches also of the golden pippin trees, which I mentioned in my former communication of 1795, being much cankered, were cut off a foot above the junction of the grafts to the stocks; and were regrafted with a new and healthy variety. Parts of the wood of the golden pippin, in which were many cankered spots, were thus placed between the newly inserted grafts, and the stocks; and these parts have subsequently become perfectly free from disease, and the wounds, previously made

Golden pippin grafted on the crab, and a crab on the graft of the pippin, with other experiments,

made by canker, have been wholly covered with new and healthy bark. These facts, therefore, satisfied me, that the debility and diseases of old varieties of fruits of this species did not originate in any defective action of the bark or alburnum, either of the root, or of the stem and branches, and my attention was consequently directed to the leaf and succulent annual shoot.

showed, that there was no defective action of the bark or alburnum.

Experiments on the leaves and annual shoots.

A few crab-stocks were grafted with cuttings of the golden pippin, in a situation and soil, where I had previously ascertained, that the wood of the golden pippin rarely remained in health at the end of a second year; and, as soon as the annual shoots had acquired sufficient growth and firmness, numerous buds of a new and luxuriant variety of apple, which had recently sprung from seed, were inserted in them. During the succeeding winter, the natural buds of the golden pippin branches were destroyed, and those inserted suffered alone to remain: and as soon as the leaves of these had unfolded, and entered on their office, every symptom of debility and disease disappeared in the bark and wood of the golden pippin: and each continued to perform its office, just as well as the wood and bark of the young seedling stocks could have done under similar circumstances. I made nearly the same experiments on the pear tree, and with the same result.

The sap circulates through the leaves, as the blood through the lungs.

I have endeavoured, in several former communications, to prove, that the sap of plants circulates through their leaves, as the blood of animals circulates through their lungs; and I have not subsequently found any facts, in the writings of other naturalists, or in my own experiments, which militate against this conclusion. I have also observed, that grafted trees, of old and debilitated varieties of fruit, became most diseased in rich soils, and when grafted on stocks of the most vigorous growth; which has induced me to suspect, that in such cases more food is collected, and carried up into the plant, than its leaves can prepare and assimilate, and that the matter thus collected, which would have promoted the health and growth in a vigorous variety, accumulates, and generates disease in the extremities of the branches and annual shoots, while the lower part of the trunk and roots remain, generally, free from any apparent disease,

disease. I am, therefore, much disposed to attribute the diseases and debility of old age in trees to an inability to produce leaves, which can efficiently execute their natural office: and to some consequent imperfection in the circulating fluid. It is true, that the leaves are annually reproduced, and therefore annually new: but there is, I conceive, a very essential difference between the new leaves of an old, and of a young variety: and in support of this opinion, I shall observe, that the external character of the leaf of the same variety at two, and at twenty years old, is very dissimilar; and it therefore appears not improbable, that farther changes will have taken place at the end of two centuries*.

The leaves the organs that first fail.

If these opinions be well founded, and the leaves of trees be analogous to the lungs of animals, is it very improbable, that the natural debility of old age of trees and of animals may originate from a similar source?—This is a question, upon which I am not by any means prepared to give an opinion: but I believe it will very generally be admitted, that the human subject is best formed for long life, when the chest is best formed to permit the lungs to move with most freedom. I have also long and attentively observed among our domesticated animals, that those individuals longest retain their health and strength, and best bear excessive labour and insufficient food, in which the chest is most deep and capacious, proportionately to the length of current the circulating fluid has to run; and the same remark will, I believe, be generally found applicable to the human species. I am, my dear Sir,

Longevity principally affected by the state of the lungs?

with great respect, sincerely yours,

Downton, Feb. 26, 1810. THO. AND. KNIGHT.

* The leaf of a seedling apple or pear-tree, when the plant is very young, is generally almost wholly free from the pubescence or down, which subsequently appears on its under surface; and which Bonnet and Mr. Mirbel have supposed to increase its surface and powers. But I feel little disposed to adopt this hypothesis, having observed, that the leaves of some new varieties of the apple, which have sprung from seeds of the Siberian crab, have both surfaces nearly equally smooth; and that these varieties grow faster, and bear heavier crops of very rich fruit, than any others, without being exhausted or injured.

Down on leaves a symptom of decline.

VI.

VI.

Remarks on Mr. DALTON'S *Hypothesis of the manner in which Bodies combine with each other. In a Letter from* JOHN BOSTOCK, *M. D.*

To Mr. NICHOLSON.

SIR,

New principle in chemistry.

IN the first part of Mr. Dalton's system of chemical philosophy, he announced the existence of a new principle, respecting the manner in which bodies combine with each other. At the time he went no farther, than merely to state the outline of his hypothesis, refering to the subsequent part of his work, for the facts and experiments from which it was derived. The second part of the system being now published, and a considerable portion of it being occupied with details, which may be considered as dependant upon, or illustrative of the hypothesis, and the truth of it being assumed as the foundation of a great part of Mr. Dalton's reasoning, we may be fairly entitled to enter into an examination of the proofs, which are adduced in its support.

Different modes of establishing a new hypothesis.

In order to assist us in forming a clear conception of the subject, it may be desirable to refer to the different ways in which we attempt to establish a new hypothesis. We may show, that it is consonant to the generally received laws of physics; that it coincides with previously admitted facts, and is deducible from them; or, proceeding without these previous steps, we may propose a new opinion, that is unsupported by analogy or induction, and afterwards endeavour to substantiate it, by experiments and observations made expressly for that purpose. To the latter of these the term *hypothesis* more strictly applies, while the former is more correctly denominated *theory*; and as our plan of proceeding in the investigation of any truth must be considerably different, according as we adopt the one or the other of these modes, it is of some importance to bear in mind the distinction, which subsists between them. It will, I think, be found, that Mr. Dalton's doctrine is altogether *hypothetical*, or one which depends for its proof entirely upon subsequent observations and experiments. By those who

who are in the habit of philosophical investigation, it will not be supposed, that any degradation of Mr. Dalton's doctrine is intended by this statement of what I conceive to be the correct view, in which it ought to be regarded; some of the most important discoveries of modern times have originated from mere hypothesis, nor does it appear that any objection can be urged against the use of it, provided only we bear in mind the nature of the ground on which it is founded, and recollect that its proof depends entirely upon the strength of the facts that are adduced in its favour.

Remarks on Mr. Dalton's terms.

Atom.

I shall offer a few remarks upon the terms that are employed by Mr. Dalton, and first upon the word *atom*, which holds so prominent a place in his pages. Considerable merit is due to him for the clear manner in which this part of the subject is brought into view, and this, I conceive, is in no small degree owing to his having employed a term, the meaning of which was established both by custom and analogy, so that it could scarcely be misunderstood *, whereas the words *particle* †, generally employed by the English chemists, and still more, *molecule* ‡, employed by the French, were obliged to be altered and retracted from their popular acceptation, before they could be employed in a scientific discussion. To the word *atom* no objection can attach. I cannot, however, assent to the manner in which Mr. Dalton has used the words *binary*, *ternary*, &c.; because, I think, it is calculated to introduce confusion into chemical language. The words themselves are modern, they are confined to the writing of the chemist, and were formed to express a peculiar constitution of bodies, yet Mr. Dalton employs the terms in a sense essentially different from that originally imposed upon them. I am not fond of proposing new terms, being aware of the difficulty of inventing such as may be unexceptionable, yet I shall venture on the present occasion to suggest, that *binate*, *trinate*, &c. may serve to denote that the substance in question consists of 2, 3, or more atoms, without the liability to errour, that

Binary, ternary, &c.

Other terms proposed for these.

* Ατομος, insecabilis, indivisibilis.

† Diminutive of pars, a little part.

‡ Diminutive of moles, a little mass.

that is involved in the words *binary, ternary*, &c., which refer, not to the quantity, but to the quality of the atoms*.

Chemical synthesis.

After these preliminary observations, I shall proceed to the subject more immediately under consideration. In the conclusion of the first part of his system, Mr. Dalton introduces a short chapter on chemical synthesis, in which he informs us, that "one great object of his work is "to show the importance and advantage of ascertaining the "relative weights of the ultimate particles, both of simple "and compound bodies, the number of simple elementary "particles which constitute one compound particle, and the "number of less compound particles which enter into the "formation of one more compound particle." He then goes on to state, that when two bodies A and B are disposed

* I hope I shall not be thought hypercritical, if I point out an inaccuracy into which you have fallen, in your translation of Mr. Grotthuss's paper on metallic arborization, inserted in your last number. The following sentence occurs in it;" "Let us suppose a thread of water formed "of three integrant particles, each composed of two atoms of oxigen, "represented by $\overset{\circ}{\circ}$, and one of hidrogen, &c." Knowing by the date of the paper, that between 3 or 4 years must have elapsed since it was written, I was surprised to meet with the word *atom* in this connexion, and was induced to turn to the original, where I found the expression to be; "Considérons un filet d' eau formé de trois molécules inté-"grantes dont chacune soit composée d' oxigène, representé par $\overset{\circ}{\circ}$, & "d'hidrogène représenté par l, &c." In works of science, particularly where the language has been controverted, we should closely adhere not only to the *sense*, but, where it is possible, to the *terms* of the original. Ann. Chim. V. LXIII, p. 19, 20. **

** [*Answer*. It appears to me, that one great source of the corruption of language is the retaining the words of one language in what is called a translation into another, without necessity; and that this has been of late years a growing evil in our own, partly from the carelessness and partly from the ignorance of translators, which I have generally been studious to avoid. To the word *molecule* I have always felt a particular objection, not only because I conceive our English word particle to be of the same signification, but because, when pronounced, its sound is liable to excite a ludicrous idea. Mr. Dalton's recent observation, that he did not understand the meaning of the word particle, induced me to employ that of atom, to which I did not imagine any objection could lie. C.]

to combine, their atoms must unite in the following order, beginning with the most simple case.

"1 atom of A + 1 atom of B = 1 atom of C, binary.
"1 atom of A + 2 atoms of B = 1 atom of D, ternary.
"2 atoms of A + 1 atom of B = 1 atom of E, ternary.
"1 atom of A + 3 atoms of B = 1 atom of F, quaternary,
"3 atoms of A + 1 atom of B = 1 atom of G, quaternary, &c."

The author then proceeds to lay down some "general rules," which, not being immediately deducible from the foregoing statement, nor, as far as I can judge, having any previous analogies in their favour, we must regard as speculative positions, the proof of which is reserved for the 2nd part of the work. The first four of these rules are as follows. "1st. When only one combination of two bodies can be "obtained, it must be presumed to be a *binary* one, unless "some cause appear to the contrary. 2nd. When two com-"binations are observed, they must be presumed to be a "*binary* and a *ternary*. 3d. When three combinations are "obtained, we may expect one to be *binary*, and the other "two *ternary*. 4th. When four combinations are observed, "we should expect one *binary*, two ternary, and one qua-"ternary, &c." In support of these positions Mr. Dalton offers no arguments, but resting their truth entirely upon their intrinsic probability, he proceeds, without any hesitation, to deduce from them the most important conclusions, and such as lie at the foundation of a great part of his subsequent reasoning.

Composition of water, and of ammonia.

From the first of the above rules Mr. Dalton draws the two conclusions, that water is a binary compound of hidrogen and oxigen, having the relative weight of its two elementary atoms nearly as 1 to 7; and that ammonia is a binary compound of azote and oxigen, the atoms of which have the weight of 1 to 5 respectively. But before we can follow Mr. Dalton through this train of consequences, there are some difficulties to be cleared up, which oppose our progress at the very outset. When bodies unite only in one proportion, whence do we learn that the combination must be binary? Why is it not as probable, that water is formed of two atoms of oxigen and one of hidrogen, of two atoms of hidrogen

Objections.

hidrogen and one of oxigen, or in short of any assignable number of atoms of hidrogen and oxigen? I do not perceive, that Mr. Dalton has given any reason in support of this binary combination, in preference to all the rest, and I am unable to conjecture what reason can be urged in its favour. If we feel a difficulty in assenting to the first of these rules, still more shall we be inclined to hesitate, as we advance to the more complex cases, which are stated in the 2nd, 3d, and 4th. It is obvious, that as there is more scope for variety in the mode of combination, in the same ratio does the probability of Mr. Dalton's supposition diminish. It is not necessary to enter into the consideration of the other "general conclusions," because the estimates which are formed of the weights of oxigen, hidrogen, and azote, as laid down in the first two, enter as elements into all the subsequent calculations. We may therefore conclude this part of the inquiry by stating, that the positions respecting the weight of the atoms of oxigen, hidrogen, and azote, as laid down in the first part of Mr. Dalton's system, are entirely hypothetical, are assumed without any authority either from analogy or induction, and that consequently, they must rest entirely upon such facts as the author brings forwards expressly for their support.

Assumptions of Mr. Dalton.

The points which Mr. Dalton has here assumed, and which remain to be proved, are the following. First, that when only one combination of two elementary bodies can be obtained, it must be *binary*; 2nd. That only one combination of oxigen and hidrogen, and only one combination of hidrogen and azote can exist; 3d. That the ratios of 1 to 7, and of 1 to 5 express the relative weights of these atoms. It may be confidently asserted, that these steps in the proof are so intimately connected together, and that the last depends so entirely upon the previous ones, that the least flaw in either of them must utterly demolish the whole hypothesis. I shall now turn to the 2nd part of the system, where the author promised that the facts and experiments should be detailed, from which the conclusions were derived. The first chapter is on "elementary principles," oxigen, hidrogen, azote, &c.; each of these individually forms the subject of a short section, in which the mode of procuring them

Elementary principles.

is

is described, and an outline is given of their chemical and physical properties, but nothing occurs, either in the way of proof or illustration of the "general conclusions." We, however, naturally expect, that we shall find the information of which we are in search, when we come to the next chapter, which professedly treats of compounds of two elements, and first of water. After giving a brief account of the experiments on the composition and decomposition of water, experiments which have been repeated in various forms, by the most distinguished chemists of France and England, and respecting which the utmost pains have been taken to ensure accuracy, Mr. Dalton remarks, that "the "general result was that 85 parts by weight of oxi-"gen unite to 15 of hidrogen to form water." In this estimate, however, he does not acquiesce; he refers to some objections that have been urged against it by Mr. Humboldt and Gay-Lussac, whose reasoning on this subject he considers as "perfectly satisfactory." They conceive, that in consequence of the aqueous vapour which gasses usually contain, a less quantity of real hidrogen must have been employed, than was assumed in the calculations, and they reduce it from 15 parts to 14·3 parts, increasing, of course, the quantity of pure oxigen to 85·7. Mr. Dalton adds, "the relation of these numbers is that of 7 to 1 nearly." He continues; "There is another consideration "which seems to put this matter beyond doubt. In Vol-"ta's eudiometer, two measures of hidrogen require just "*one* of oxigen to saturate them. Now, the accurate expe-"riments of Cavendish and Lavoisier have shown, that "oxigen is nearly 14 times the weight of hidrogen; the ex-"act coincidence of this with the conclusion above deduced "is a sufficient confirmation." Notwithstanding the acknowledged sagacity of the author, I cannot but consider the above remarks as totally irrelevant to the question under discussion. The facts that are adduced refer merely to the relation which the bulk of hidrogenous gas bears to its specific gravity, and have no relation to either of the fundamental points in Mr. Dalton's hypothesis.

Compounds of two elements. Water.

Having now gone through the chapter in which the promised facts and experiments are contained, it may be proper to

The facts do not support

the assumptions.

to consider how far they substantiate and authorize the conclusions that have been deduced from them. Nothing, as I can judge, has been advanced, which has the least reference to the first position, that when only one combination of two bodies can be obtained it must be *binary*. This therefore, which is the foundation of all the argument, must be regarded as a mere postulate, unsupported by facts, and not resting immediately either on analogy or induction. The 2d position, that only one combination of oxigen and hidrogen, and only one of hidrogen and azote can exist, rests rather on a different foundation from the preceding. We have never yet been able to produce more than one combination with each of these substances, therefore Mr. Dalton concludes, that only one combination can possibly exist. But it is evident that this is not a legitimate way of reasoning; it amounts to no more than a presumption, and that drawn from our ignorance; and is in contradiction to the general analogy of chemical affinities*. From what has been stated I think it will be admitted, that Mr. Dalton's hypothesis fails entirely in its two fundamental positions, that one of them is entirely without support, and that the other is founded upon a presumption which scarcely amounts to a probability. In this state of the case we might perhaps be justified in discarding the hypothesis altogether, or at least in requiring the author to bring forward some direct arguments in its favour, before we should pay any farther attention to it. I shall, however, pursue a different course, and shall examine how far the consequences would fairly follow from the premises, supposing that these were demonstrated. Nor is this investigation to be considered as a matter of mere speculative curiosity, because, although the hy-

Are the consequences fairly deducible from the premises?

* Although in forming his general conclusions Mr. Dalton assumes, that only one combination of oxigen and hidrogen can exist, and from this circumstance argues that water is a binary compound, and hence deduces the weight of its atoms, *a calculation, upon which all the details of the hypothesis depend*, yet in the 2d part of his system he endeavours to show, that the fluoric, muriatic, oximuriatic, and hyperoxmuriatic acids are all compounds of oxigen and hydrogen, and even forms estimates of the number of atoms of each element which enters into their composition. So dangerous is it for the most acute mind to indulge in merely speculative reasoning!

pothesis

pothesis is at present unsupported by any evidence, yet it implies nothing contradictory to reason or experience; so that, if it appear that there are any number of facts which coincide with it, or that are conveniently explained by it, they must be considered as forming an indirect argument in its favour. Now if we admit the truth of the hypothesis, so far as respects the union of the two elements atom by atom, a necessary consequence is, that the compound will be either *binary**, or that the number of atoms in the substance which exists in the greatest quantity will be some multiple of the atoms existing in the smaller quantity. I will take the example of sulphur and oxigen. These bodies unite in three different proportions, of which one must be *binary*, i. e. 100 atoms of sulphur and 100 atoms of oxigen will unite and form 100 atoms of some compound, which compound we might a priori suppose would be that in which the strongest affinity is exerted between the two elements. In all the other compounds it necessarily follows, that the number of atoms which exist in the largest quantity, must be either as 200, 300, 400, or some multiple of 100; for it is obvious, that the substances could not combine atom by atom, were the greater number of atoms to exist in the ratio of 120, 150, or in any quantity intermediate between the centesimal numbers. It seems the most natural to regard the sulphuric acid as the *binary* compound of sulphur and oxigen, or that 100 atoms of sulphur and 100 atoms of oxigen form 100 atoms of sulphuric acid. According to the most accurate experiments, the respective weights of the component parts of sulphuric acid are 100 parts of sulphur to 136·5 parts of oxigen†, therefore the weight of an atom of sulphur will be to the weight of an atom of oxigen as 1 to 1·365, or nearly as 3 to 4. Let us now try how this supposition will coincide with the composition of the next compound of sulphur and oxigen, sulphurous acid, 100 parts of which are stated, upon the respectable authority of Dr.

Compounds of sulphur and oxigen.

* The word *binary* is here, and in all the other parts of this paper, employed in the sense which Mr Dalton assigns to it; but it is obvious, how much confusion may be introduced by the circumstance of there being two different significations applied to the same scientific term.

† Klaproth, Gehlen's Jour. V. 109.

Thomson,

Compounds of sulphur and oxigen.

Thomson, to consist of 53 parts of sulphur and 47 of oxigen, or 100 parts of sulphur to 88·6 of oxigen*. Now in this case the proportion of oxigen to the sulphur is diminished, therefore the 100 parts of oxigen, which entered into the composition of sulphuric acid, will be combined with a greater number of atoms of sulphur, and the next smallest quantity with which they can unite is the next multiple of 100, that is 200. Sulphurous acid will therefore be composed of 100 atoms of oxigen, and 200 atoms of sulphur. Now supposing the weight of an atom of sulphur to be to an atom of oxigen as 1 to 1·365, the weight of the two atoms of sulphur in sulphurous acid will be to the single atom of oxigen as 2 to 1·365, nearly as 6 to 4, so that 100 parts of sulphuroua acid will consist of 60 parts of sulphur and 40 parts of oxigen. The third compound of sulphur and oxigen is the oxide of sulphur, and this is stated, upon the same authority with the last†, to be composed of 100 parts of sulphur with 7 of oxigen. Here as before we are to take the oxigen as the fixed quantity, and to find some multiple of 100 which may express the proportion of sulphur. Now if we suppose that one atom of oxigen unites with 19 atoms of sulphur, to form the oxide of sulphur, or that 100 atoms of the oxide of sulphur are composed of 100 atoms of oxigen and 1900 atoms of sulphur, we shall find, by following the same method of calculation, that the weight of sulphur to that of oxigen will be very nearly as 57 to 4. With respect to sulphur therefore it appears, that one case is favourable to the hypothesis and the other adverse to it; for the analysis of the sulphurous acid makes the sulphur to the oxigen as 53 to 47, while the proportion of 57 to 4, for the oxide of sulphur, agrees very nearly with Dr. Thomson's estimate of 100 to 7‡.

* Thomson, I. 87. † Thomson, I. 88.

‡ As the oxide of sulphur has probably a real existence, and as the numbers which Dr. Thomson has assigned for its constituents coincide remarkably with Mr. Dalton's hypothesis, I have given it a place in the calculations, although my opinion is, that we have not yet sufficient grounds for forming an exact estimate of the proportion of its elements, and the above coincidence is purely accidental. See Dr. Thomson's paper in Nich. VI, 101.

I will

I will now apply the same mode of examination to the compounds of oxigen and carbon, carbonic acid and carbonic oxide. According to the hypothesis, one of these must be binary; let us suppose that it is the carbonic acid which is in this state, or that 100 atoms of carbonic acid consist of 100 atoms of oxigen and 100 atoms of carbon. But the carbon in carbonic acid is to the oxigen as 28 to 72, or nearly as 100 to 257, therefore the atom of carbon will be to the atom of oxigen as 1 to 2·57. In the oxide of carbon the quantity of oxigen being diminished, we must take some multiple of 100, to express the ratios of the increased quantity of carbon. If we suppose that two atoms of carbon unite to one of oxigen, it will make the proportion 2 to 2·57. This will give the constituents of carbonic oxide to be 100 parts of carbon to 128·5 of oxigen, whereas, from the analysis of this substance we learn, that 100 parts of carbon require 146 parts of oxigen to convert them into carbonic oxide*. I have here assumed, that carbonic acid is the binary compound, but I will now reverse the supposition, and make the oxide of carbon to consist of 100 atoms of oxigen and 100 atoms of carbon. According to this principle the weight of the atom of carbon will be to the atom of oxigen as 1 to 1·46, and proceeding as above for the constituents of carbonic acid we shall have 100 atoms of the acid to consist of 100 atoms of carbon and 200 of oxigen, making a ratio nearly of 1 to 2·9, which give the constituents of carbonic acid to be 100 parts of carbon to 290 parts of oxigen, whereas we find by analysis, that 100 parts of carbon require only 257 parts of oxigen to be converted into carbonic acid†.

Compounds of carbon and oxigen.

I will now examine in the same manner the compounds of oxigen and azote. I will, in the first place, assume that nitric acid is the binary compound, and as 100 parts of azote require 236 parts of oxigen to be converted into nitric acid‡, it follows that the atom of azote is to that of oxigen as 1 to 2·36. Nitric oxide receiving an addition of azote, the proportion will be as 2 to 2·36, which will make the constitution of the oxide to be as 100 to 118, whereas it is stated to

Compounds of oxigen and nitrogen.

* Thomson, II, 150. † Thomson, II, 149. ‡ Thomson, II, 153.

be in the ratio of 100 to 136*. With respect to the nitrous oxide, the ratio seems favourable to the hypothesis, for if we suppose, that 100 atoms of oxigen are combined with 400 atoms of azote, to produce 100 atoms of nitrous oxide, the proportion of the constituents would be in the ratio of 100 to 59, which is very nearly what it appears to be from analysis†. Here, therefore, one case appears to be favourable, and the other adverse to the hypothesis.

I shall now reverse the above calculations, and assume that nitrous oxide is the binary compound, which Mr. Dalton supposes to be the case. On this principle the atom of oxigen will be to the atom of azote nearly as 100 to 170, or as 1 to 1·7. The constituents of nitric oxide will then be in the ratio of 100 to 117·6, and of nitric acid as 100 to nearly 233. I have only admitted three compounds of oxigen and azote, because, even although we suppose nitrous acid, and oxinitric acid to be substances that are always uniform in their composition, a supposition which is at least doubtful, it seems natural even on Mr. Dalton's hypothesis, to regard them as secondary compounds, formed by the union of a certain number of atoms of nitric acid with a certain number of atoms of azote, in the one case, and of oxigen in the other.

The facts do not bear out the hypothesis.

I shall not think it necessary to pursue this examination any farther, the examples which have been adduced are sufficient to establish the conclusion, that although the facts occasionally coincide with the hypothesis, they more frequently oppose it. It will perhaps be said, that these analyses are not correct, and I am far from denying that this may be the case, although I have purposely selected these, which appear to stand upon the best foundation. But here we must recollect the difference between a theory and an hypothesis: had it been shown by a previous train of reasoning, that Mr. Dalton's opinion was supported by any strong analogy, or that any poweful arguments could be adduced in its favour, we might then have been permitted to adjust the facts to the theory. As the question now stands, however, we are to prove the hypothesis solely from the facts, and therefore we are not to object to the facts merely be-

* Thomson, II, 168.

† Ibid.

cause

cause they do not coincide with the hypothesis. Mr. Dalton must first prove, that the analyses are incorrect, and must rectify them; he must then compare them with the hypothesis, and if they generally coincide with it, he may adduce them as arguments in its favour. In the mean time, taking the facts as they now exist, I think we are warranted in concluding, that they are not favourable to the hypothesis.

Atoms may unite in various numbers.

After this examination of Mr. Dalton's arguments in support of his opinion, I shall conclude with some farther observations upon the same subject. In the first place, although it be admitted, that when bodies unite, a union must take place between the individual atoms of which they are composed, yet it does not follow, that this union must take place atom by atom, it does not follow, that one atom of A must unite with one atom of B, or with two atoms of B, &c.; I conceive it equally probable, that two atoms of A may unite with three atoms of B, four atoms of A with five atoms of B, in short, that any indefinite number of atoms of A may unite with any indefinite number of atoms of B. By this admission we should avoid one of the most embarrassing circumstances in Mr. Dalton's hypothesis, as it now exists, namely the necessity of the greater number of atoms being always some multiple of the less. This idea is perfectly compatible with the supposition, that a certain number of atoms of A have a greater tendency to unite with a certain number of atoms of B than with any other number, although it does not previously inform us what the ratio is between these two uumbers.

Weight of component atoms not deducible from the proportions of a compound.

My next remark refers to the method, in which the weight of the atoms of two bodies is attempted to be estimated from the quantity of each of them that enters into their compounds. Admitting with Mr. Dalton, that a quantity of water consists of 7 parts of hidrogen and one of oxigen, are we to conclude that there are an equal number of atoms of these two substances, but that the atom of oxigen is 7 times heavier than the atom of hidrogen, or that there are 7 times as many atoms of the one kind as of the other? Mr. Dalton has adopted the former of these suppositions, whereas the latter appears to me the most probable. In forming an idea respecting the weight of an atom, we must

 proceed

proceed upon the same principles as in other cases. Now what is generally assigned as the cause of the different specific gravities of bodies? Why is a cube of lead heavier than a cube of ivory? The most natural answer to this question is, that the particles of lead are nearer together, and therefore a greater number of them will be contained in any given space. Whether this difference in proximity be occasioned by interposed caloric, or any power of repulsion, independent of caloric, it is not our business to inquire. Now if we endeavour to form a conception of a single particle, or what has been denominated an atom, as a substance not made up of any smaller parts, and which cannot therefore be under the influence of any repulsive power, so far as its individual existence is concerned, it follows that atoms of all kinds must have the same specific gravity. If then they differ in their weight, there must be a corresponding difference in their size, an atom of oxigen for example must be 7 times as large as an atom of hidrogen; but were this the case it could scarcely be regarded as an atom, but as a substance of considerable extent, capable of being divided into still smaller parts.

Cause of specific gravity.

These observations, it is obvious, are entirely conjectural, and I have only brought them forward, to meet conjectures on the other side of the question. My object in this paper has been to point out the insufficiency of Mr. Dalton's hypothesis, without attempting to substitute another in its place; for important as the object is which he has endeavoured to accomplish, and ingenious as the attempt may appear, I am decidedly of opinion, that we have not yet a sufficient basis of facts, on which to erect so capacious a superstructure.

I am, Sir, yours &c.

J. BOSTOCK.

Knot'shole Bank, near Liverpool,
Feb. 23*d.* 1811,

VII.

VII.

Effect of Changes in the State of the Atmosphere on Mr. De Luc's *Electric Column. Communicated by* Thomas Forster, *Esq.*

To Mr. NICHOLSON.

SIR,

I TAKE the liberty, with your permission, of communicating to the public, through the channel of your Journal, a curious coincidence. The electric column of Mr. De Luc, having been described in your pages, is of course well known to your readers. The two bells attached to the *plus* and *minus* end of the column of an instrument of this kind, kept by a relation of mine at Walthamstow, after having continued ringing (in the manner which has been described in your journal) for several months, began on the 4th of September last to pulsate very irregularly, and at long intervals. This irregularity continued through the month of September, and until the 16th of October, when they ceased to ring entirely. During the period of their hurried and irregular pulsation, that is, from the 4th of September to the 16th of October, a very peculiar kind of weather prevailed, strong easterly winds, very clear nights abounding with small meteors commonly called falling stars, the hygrometer rendered useless by the excessive dryness of the air, and the varied and multiform appearances of the *cirrhus* and *cirrho-stratus* clouds, (not followed by rain, as usual) marked a very remarkable state of the atmosphere. On the 16th of October, the day on which the bells stopped ringing, a remarkable change of weather became very evident, a strong current of air from the South, and the prevalence of the *spotted cirrho-stratus* cloud indicated the approach of rain, which ensued during the night, and rainy weather for several weeks succeeded. About a week ago the same kind of weather, which prevailed between the 4th of September and the 16th of October last, returned, the air became extremely dry, the wind became easterly, and very strong, the small meteors have been again observed at night, and the same kind

Connection of the state of the atmosphere with the action of Mr. De Luc's electric column.

kind of *cirrhus cloud* has prevailed, indicating a great disturbance of the electric state of the atmosphere; *and the bells, after having pulsated very irregularly for a week, have again completely stopped.* Thus there seems to be a connection between the *electric* state of the air, (indicated by the abovementioned circumstances) and the phenomena exhibited by Mr. De Luc's column, which is a circumstance to which Mr. De Luc wished to direct the attention of meteorologists, which has induced me to request the insertion of these observatious in your interesting journal.

I remain, sir, yours, &c.

THOMAS FORSTER.

Clapton, Hackney,
March 18, 1811.

March 19.

P. S. It is remarkable, that the weather has completely changed again to day: on getting up this morning, I observed the *cirrho-cumulus* cloud through the mist which covered the ground, the hygrometer indicated an increased moisture of the air; and *the bells of Mr. De Luc's column again ring with a regular but weak pulsation.*

VIII.

On the Nature of Oximuriatic Acid, in Reply to Mr. JOHN DAVY. *By* J. MURRAY, *Lecturer on Chemistry, Edinburgh.*

To Mr. NICHOLSON.

SIR, *Edinburgh, March* 11*th*, 1811.

I SHALL beg your permission to occupy a few pages of your Journal with some remarks on the reply of Mr. J. Davy, in your last number, to the observations which I had offered on Mr.Davy's opinion of the nature of muriatic and oximuriatic acids.

Theory and hypothesis confounded.

T gentleman first remarks, that I have not attended to the distinction between theory and hypothesis, but have made promiscuous use of the two words, and have hence taken

taken an incorrect view of his brother's opinion, which, he adds, is a theory, not an hypothesis.

General use of the word theory.

The free signification generally given to the word theory in chemical language is so well understood, that I did not suppose it could have given rise to any ambiguity requiring to be pointed out. Theory, strictly understood, implies, no doubt, a principle established by just induction from individual facts, and applied to the explanation of phenomena; while in an hypothesis a principle is assumed, whence phenomena are attempted to be explained. But there is an intermediate kind of reasoning or speculation, in which there is partly generalization, partly hypothetical assumption—a principle being professedly inferred by induction, but requiring the aid of hypothesis to apply it to all the phenomena connected with the subject. To this the terms opinion, theory, and hypothesis are often indiscriminately applied; and theory in chemical language has more frequently this signification than any other. The successive revolutions in chemistry would lead us to doubt indeed if it can be often justly used in its more strict sense, implying a perfect induction, the certainty of which subsequent discoveries cannot change. The theory of Stahl, as it has been named, which at one period commanded universal assent, was quickly subverted. Much of the theory of Lavoisier, ample and conclusive as the evidence appeared to be on which it is founded, must, if late speculations be just, share the same fate. And even those more partial inductions, which appeared to have the utmost certainty, are many of them, it now appears, doubtful. To a philosophic inquirer this may perhaps suggest some caution in applying the term theory in its strict signification, and it may guard him against the most common of all errours—an undue confidence in our speculations, and the belief that the opinions of our day are demonstrated truths.

Source of Mr. J. Davy's mistake.

Mr. J. Davy's mistake, and which I should have obviated in my former paper, had I supposed any one attending particularly to the subject, and accustomed to scientific deduction, could have fallen into it, and had I not wished to avoid holding out unnecessarily Mr. H. Davy's opinion as purely hypothetical,

hypothetical, is the supposing this opinion to be a theory in the strict sense of the term. In stating the grounds of this opinion, he exposes too very clearly the source of his errour. Mr. Davy, he remarks, "combines oximuriatic acid gas with hidrogen gas, and forms muriatic acid gas. In his theory, muriatic acid gas is a compound of oximuriatic gas and hidrogen. He combines oximuriatic gas with sulphur, phosphorus, and the metals; and in his theory the resulting substances are compounds of the inflammable and metallic bodies respectively, and oximuriatic gas. Here we perceive no supposition, but a simple expression of facts, and this I humbly conceive is pure and genuine theory." It is of main importance Mr. Davy also remarks, "that expression of facts be not misrepresented. It is of great consequence that things be not termed notions, that theory be not considered as speculation."

Simple enunciation of the facts.

The proper expression of the above facts is, that from the mutual action of oximuriatic gas and hidrogen, muriatic acid gas is obtained, that from the mutal action of oximuriatic gas and inflammables, or metals, such and such substances are formed. That muriatic acid is a compound of oximuriatic gas with hidrogen, or that these substances are compounds of oximuriatic gas with the respective metals or inflammables, are inferences, which may be true, or may be false. They appear no doubt to be the most direct inferences, but the most obvious and direct conclusion from an experiment may not always be the just one. The shortest way of convincing Mr. J. Davy of this will be to state to him in the very form of expression, which he employs in the above quotation, conclusions which he cannot admit. I combine, I may say, oxide of mercury and muriatic acid, and form calomel. I conclude therefore, that calomel is a compound of muriatic acid and oxide of mercury. I combine muriatic acid and potash, and by dissipation of the water I obtain a solid product, which I consider as a compound of the muriatic acid and potash: and I perceive in these conclusions no supposition, but a simple expression of facts. Mr. Davy will in this however soon correct me, and inform me not merely that they are suppositions, but conclusions altogether false. He will perceive therefore, that the

Inferences deduced from these may or may not be true.

inference

inference which appears most obvious and direct is not always just, that it is sometimes necessary to take a more extensive or deep view, and will perhaps be convinced, that he has erred in the notion he has formed of the kind of induction, which constitutes a genuine theory.

Errour from taking into consideration only part of the facts.

The cause of errours such as this is, and which has long been recognized as the fertile source of false speculations in physics, is the taking into view only part of the facts which belong to the subject—those which appear most favourable to our induction, instead of taking the whole into consideration, and from this general view forming the most probable conclusion. If the facts above quoted from Mr. Davy were all that are connected with the question, his conclusions might appear to be just. But there are others equally connected with it, to explain which, various hypothetical assumptions must be made, the probability of which ought to be considered, and the whole compared with any other induction that may be formed.

Formation of oximuriatic acid from muriatic acid and oxide of manganese.

Thus in distilling muriatic acid from black oxide of manganese, the oxide loses a portion of its oxigen, the muriatic acid disappears, and oximuriatic acid is obtained. The direct conclusion from this experiment (and it is equally direct with Mr. Davy's conclusion from the experiment of detonating oximuriatic gas and hidrogen) is, that the oxigen of the oxide has combined with the muriatic acid, and formed the oximuriatic; and to obviate this he is obliged to *suppose*, that the oxigen of the oxide combines with the hidrogen of the acid, forming water, and setting free the oximuriatic acid. If we expose liquid oximuriatic acid to solar light, oxigen is expelled, and muriatic acid remains; and the most direct inference from this is, that oximuriatic acid is a compound of muriatic acid and oxigen. Mr. Davy must have recourse to a less obvious explanation, and *suppose* a portion of water to be decomposed, its oxigen disengaged, and its hidrogen combined with the oximuriatic acid. And he has no proof of the formation and decomposition of water in these experiments, farther than that they must take place, if his theory be true.

Both explanations hypothetical.

If we consider therefore these two opinions under different aspects, if we set out as it were from different points, each

Both explanations hypothetical.

each will appear an induction, which to apply to all the phenomena farther requires some hypothetical assumptions. Considering the experiment of the production of oximuriatic acid, by distilling muriatic acid from substances which impart oxigen, the most direct conclusion is, that it is a compound of oxigen and muriatic acid, but to apply this in explaining the agencies of both acids, it is necessary to suppose in some cases changes to occur: the formation or decomposition of water for example, of which we have no independent proof. Considering on the other hand the experiment of the mutual action of oximuriatic and hidrogen gasses, the most direct inference is, that muriatic acid is a compound of these substances; but in adopting this as the basis of a theory, it is equally necessary to advance suppositions, and in particular to suppose without any actual proof, that in different cases water is formed or decomposed. These suppositions are not always required in the two systems in relation to the same fact; but still, wherever an hypothesis is required in the one, a corresponding hypothesis will be found necessary in some part of the other. They occur on the whole just as frequently in Mr. Davy's system as in the opposite one, and they are as little supported by actual proof. He, for example, has no better proof, that water is formed when muriatic acid is distilled from black oxide of manganese, than I have of its formation in the mutual action of oximuriatic gas and hidrogen, or that it is decomposed when oximuriatic acid is exposed to solar light, than I have of its decomposition when a metal is acted on by muriatic acid gas. The two opinions are perfectly alike with regard to the evidence on which they rest derived from the above facts; and to select in one of them that part where the induction appears direct, and take it for granted that is true, the hypothetical assumptions, which must be farther made, necessarily follow: but to pursue the reverse method with regard to the other, to represent it as an hypothesis, by bringing forward the parts which require the assumption of hypothesis, and neglect or reject the more direct induction, is a mere sophism. This is exactly what Mr. J. Davy does, no doubt without being aware of it, his errour arising

arising from the confined and partial view he has taken of the subject.

All this is so obvious, that some apology is due to your readers for having illustrated it at any length. This has however been rendered necessary by the tone this gentleman has assumed through the whole of his observations. He has held out the opinion of his brother as a genuine theory resting on indubitable evidence, and has thought himself at liberty to represent the reasoning I have employed as mere speculation, and the explanations I have given as uniformly hypothetical, and having on this account no pretensions to be put in contrast with the others. He will now perceive, that the subject may be presented under a different light; and he will not perhaps again hazard the assertion, that his brother's "conclusions are not tainted by the slightest admixture of hypothesis." Least he should, permit me to add one or two illustrations, which are besides not unconnected with the subject.

Decomposition of nitrate of mercury by muriate of soda.

On adding to nitrate of mercury, muriate of soda, nitrate of soda and calomel are formed; chemists have therefore been accustomed to conclude by rules of evidence, which they thought sufficiently certain, that the nitric acid combines with the soda, and that the muriatic acid combines with the mercurial oxide, forming the calomel. According to Mr. Davy's opinion however, though the nitric acid unites with the soda, the muriatic acid does not unite with the oxide, but these substances decompose each other, the oxigen of the oxide unites with the hidrogen of the acid and forms water, and the calomel is a compound of oximuriatic acid and metallic quicksilver.

Muriatic acid and potash.

Again, on adding muriatic acid to potash we form muriate of potash, and on exposing this product to heat, so as to obtain it dry, we have hitherto believed, apparently on very strict induction, that the water was expelled, and that the dry product is a compound of muriatic acid and potash. But in Mr. Davy's new system it is supposed, that the acid and the potash decompose each other, the oxigen of the one and the hidrogen of the other combine and form water, leaving a compound of oximuriatic acid and potassium. On dissolving this in water new changes occur, the water is decomposed, the potassium receives

receives oxigen, the oximuriatic acid, hidrogen, and a compound of muriatic acid and potash is again formed. Who does not perceive in all this abundance of hypothetical assumptions? asumptions I have no hesitation in saying more gratuitous and more complicated, than any required in the opposite system, or perhaps in any other chemical speculation.

Comparison of the two hypotheses.

Mr. Davy's opinion then I regard as an hypothesis: the opinion I have maintained I have also distinctly admitted in my former paper to be an hypothesis: each rests on an apparently probable induction, and is capable of being applied with more or less probability to the various phenomena. Placing them on this ground I considered and still consider the common doctrine as superior in simplicity, in requiring less strained and less complicated assumptions, in affording explanations of facts which the other does not explain, and in according with the general system of chemical theory; receiving therefore all the support which the evidence on which that theory rests can give, while the other is anomalous, and has the weight of that evidence against it. I proceed to offer a few observations on some of the more particular topics of this discussion.

Real acids spoken of though not exhibited separately.

Mr. Davy remarks, that when I speak of muriatic acid I do not mean the whole ponderable part of it; water I consider as necessary to its existence in its gaseous state, and that it is to the substance free from this water that the term muriatic acid is applied. This is my meaning. But when he proceeds to the interrogations, has Mr. M. examined it in its insulated state? has he described its properties? or has any chemist ever obtained it? And when from the negative given of course to these he concludes, that I must regard muriatic acid gas as a compound of an unknown basis and water, he raises or magnifies difficulties of little importance, or at least not peculiar to this investigation. When I speak of real sulphuric or nitric acid, I mean the acid free from water, though it may not have been obtained in that state; and in thus using language which is familiar to chemists I am not aware, that I introduce any novelty of hypothesis. The real muriatic acid I consider as an acid from having reason to conclude, that it exists in its solid compounds,

pounds, neutralizing the bases with which it is in combination; in the same manner as I consider real sulphuric or nitric acid as an acid from knowing, that they exist in combinations free or nearly so from water, exerting a similar neutralizing power. Mr. Davy may if he pleases suppose sulphuric or nitrid acid to be a compound of an unknown basis and water, and conclude, that the supposition of the existence of these acids free from water is "speculation in the strictest sense of the word." But no chemist will find any difficulty in this. The apparently greater influence of water on the chemical powers of muriatic acid than the other acids probably arises from the facts with regard to the latter being more fully developed. On this subject I am at present engaged in an experimental investigation.

Improbability of two acidifying principles.

Mr. Davy proceeds to examine the objections which I had offered to his brother's opinion. To the remark, that there is some improbability in the hypothesis of two acidifying principles, he replies, that the only legitimate mode of reasoning is induction; and that we are not to judge of Mr. Davy's views by considering their probability or improbability a priori, but by a reference to facts. The observation would be just, were Mr. Davy's opinion a theory; but while it is an hypothesis the objection retains its force, for of two hypotheses proposed on any subject, it is fair in contrasting them to bring into view the probability a priori of the principle of the one compared with that of the other. The concession that he makes, that there is properly no acidifying principle, is rather more in point, and I have remarked in my Elements of Chemistry, in giving a view of the theory of acidity, that the supposition of acidity being a property rather derived from the base of acids than from their common principle is most conformable to Mr. Davy's speculations. Still the similarity of oximuriatic acid to oxigen so as to class them as acidifying principles is very remote; for allowing it to combine directly with any substance, it forms no acid in the common definition of the term, or any substance analogous to acids, but in its supposed combination with hidrogen, the subject of dispute; and the acidity of this, and its analogy to the common acids, are indeed such as to afford on the other hand as striking an

Perhaps none.

anomaly

anomaly in comparison with its other supposed combinations.

Want of action between charcoal and oximuriatic acid.

To the explanation I had given of the want of action between charcoal and oximuriatic acid, that it is owing partly to the absence of the water required in the constitution of muriatic acid gas, and partly to the absence of that disposing affinity exerted by the acid in establishing other combinations, Mr. Davy replies, that it would be less unobjectionable were it less hypothetical; that I do not prove muriatic and carbonic gasses contain water, I only suppose it—To this mistake that the one opinion is a theory, the other an hypothesis, I have been forced to recur so often, that it is scarcely necessary to point it out. Both are hypotheses, and if I can explain a fact by the principle of the one, which admits of no explanation by the principle of the other, the superiority of the former is with regard to this point sufficiently established—Besides, the application of the doctrine of disposing affinity alone affords an explanation of this fact. I consider it therefore as still giving a superiority to the common system, for nothing can appear more anomalous than that of all inflammable and metallic substances none but charcoal remains unchanged by oximuriatic acid, and nothing can be more satisfactory than to have a cause assigned for this. Mr. Davy considers it as of little importance, though it is this apparent anomaly, and the supposed difficulty of accounting for it according to the old opinion, which gave rise to the new doctrine, or at least first suggested the suspicion, that oximuriatic acid does not contain oxigen; he is satisfied with the observation, that we have no right to expect from a theory the explanation of ultimate facts; that is, not of comprehensive facts arrived at by a generalization which cannot be carried farther, for the fact in question is not of this kind, but of individual facts which the theory will not explain; a very safe conclusion, in which it may be proper for him to abide. This, he adds, is an ultimate fact, "one of those which constitute as it were the axioms of the science;" and he adds, that he is glad it is not tortured by hypothetical explanation. How it is elevated to an axiom, taking this word in its common sense of a fundamental proposition, self-evident, and therefore not admitiing

of

of demonstration, I do not understand; at least I do not perceive how this definition applies to the proposition, that oximuriatic acid will not combine with charcoal. Could Mr. Davy discover even a supposition by which this fact could be explained, I have little doubt that he would gladly remove it from the class of axioms, and from the facility with which he admits so many other suppositions, that he would submit it even to some degree of torture, to bend it into conformity with the system.

Action of water in favouring the evolution of carbonic acid.

On the fact with regard to the influence of water in favouring the disengagement of carbonic acid from bases with which it is combined, it is scarcely necesssry to make any observation. I was aware, that it might operate by its affinity to the base, for I had suggested this some years ago, in the discussion of the question with regard to the influence of water on the constitution of elastic fluids, in my chemical system. But it farther appeared to me probable, that it operates likewise by its affinity to the acid, as there is reason I think to suspect, that the agency of water is important with regard to all acids. It is still however a case in point, since it is an example of the powerful agency of water in modifying affinities, and of a decomposition not being capable of being effected unless it be supplied; as well as a proof that a body, whether it be an earth or an acid, cannot be obtained insulated from its combinations, unless it receive the portion of water which it requires.

Some water retained in muriatic acid gas from its powerful affinity for it.

The argument, that from the strong affinity of muriatic acid to water a portion of it will be retained in combination with it in the elastic state, I hold valid, notwithstanding the reply, that muriatic acid does not exist as a gas in combination with different proportions of water, "for the proportion of hidrogen gas produced from muriatic acid gas, acted on by different metals, is always the same, whether it has been exposed to the influence of drying salts or not." The inference which would render this reply conclusive does not follow; admitting that muriatic acid gas cannot exist with different proportions of water, the only inference then would be, that muriatic acid gas obtained by the usual methods is in as dry a state as we can obtain it insulated. But I doubt altogether the alleged fact; for Dr. Henry found by

by the more unexceptionable mode of the action of electricity, that recent muriatic acid gas affords $\frac{1}{11}$ of its bulk of hidrogen, while after the full action of muriate of lime it affords only $\frac{1}{33}$ of its bulk. The inference in the reasoning on this subject, from the affinity of potassium to oxigen, arises from a misconception of the theory of Berthollet, on which the original argument is founded, so obvious that it does not require to be pointed out.

The author's experiments accurate.

Mr. Davy proceeds to some observations on my experiments, on which I shall offer few remarks, as I shall probably have an opportunity of engaging in the discussion of this part of the subject, to greater advantage, when better acquainted with the experiments brought forward in opposition to those I have stated. I may be allowed to say, that of the accuracy of the results I obtained, particularly of those which appear to be questioned, the formation of carbonic acid, when carbonic oxide, hidrogen, and oximuriatic acid gasses are submitted to mutual action, I am fully convinced. I had preserved the notes of these experiments, some of them written by Mr. Ellis, others by myself, at the time they were made; and in all of them carbonic acid was formed, though there often remained a sensible quantity of carbonic oxide, of which I have taken notice in my paper. How Mr. Davy infers, that the whole carbonic oxide should be converted into carbonic acid, in order to admit the inference, that oxigen is communicated from the oximuriatic acid, I do not comprehend. It is sufficient, if a portion of carbonic acid is produced. No notice perhaps is due to the remark, that in one of my experiments a small residue of common air was observed, whence it is inferred, that the conversion of carbonic oxide into carbonic acid might be owing to the presence of atmospheric air. Carbonic oxide gas requires for its conversion into carbonic acid by detonation with atmospheric air a quantity equal at least to $2\frac{1}{4}$ of its bulk; and not a twentieth of this quantity could have been introduced even in the most inaccurate mode, in which Mr. J. Davy may suppose the experiment to have been performed. The small portion observed had originated from the minute quantity disengaged by the force of the detonation from between the sides of the tube and the quicksilver,

silver, which, added to what might be disengaged by the transmission through the water, and agitation with it after the explosion, and to the slight admixture to which all elastic fluids are liable, (expressed in one of Mr. Davy's own experiments, by the phrase of "no more impurity than might be expected in the air in the gasses), had formed a quantity capable of being discovered. It could obviously detract nothing from the conclusiveness of the experiment, independent of the circumstance, that it was not observed in the more decisive experiment of the slow mutual action of these gasses.

He is willing to admit the accuracy of my experiments, so far as they relate to the want of action between carbonic oxide and oximuriatic acid, when water is excluded. This is a fact, which, though it presents an anomaly in the new hypothesis, is not absolutely hostile to it; and I have, it seems, "very satisfactorily proved" this nonaction, and that without the agency of water no carbonic or muriatic gas is formed. But my experiments, which afford results presenting a difficulty in the theory, are it seems inaccurate; and beside endeavouring to make this apparent by various observations, Mr. Davy informs us, that either alone, or in conjunction with his brother, he has, if not repeated them, (which would have been the most direct mode of proving their inaccuracy), at least made experiments of a similar kind, and with very different results. Thus he has performed some of the experiments of Cruickshank on the carburetted hidrogen gasses. The source of fallacy, which I supposed would have been contended for, in the presence of a portion of oxigen in the composition of these gasses, he seems to think of little importance; the principal source of fallacy is supposed to be in the presence of water, and he therefore made the detonations over mercury, and "never obtained carbonic acid gas, though oximuriatic gas in great excess was employed." I shall not say any thing of the source of fallacy, which I conceive has given rise to this observation, until I have repeated the experiment with the necessary precautions. I shall only observe, that Mr. Davy does not directly deny, that carbonic acid is formed, he has only performed the experiment without obtaining it, and he does not inform his readers

Mr. Davy's experiments.

readers what were the actual results. The hidrogen must of course have been removed by the action of the oximuriatic acid: What became of the carbon? Was it precipitated in the state of charcoal? Or did it remain in the state of carbonic oxide? Or did the gasses form a ternary combination? and how was the absence of carbonic acid established? Yet with all these deficiencies a result thus generally stated is brought forward in opposition to experiments minutely detailed. Neither does he deny the formation of carbonic acid in my experiment of submitting carbonic oxide, hidrogen and oximuriatic gasses to mutual action: he supposes its formation owing to the presence of atmospheric air, as I have already stated, or to the introduction of water after the experiment; the gratuitous assumption being made of the formation of a triple compound of carbonic oxide, hidrogen, and oximuriatic acid, by which this water would be decomposed. Hence, in the experiment made in conjunction with his brother, ammonia was used instead of water to absorb the muriatic acid, and it is inferred, that no carbonic acid was formed. It is singular however, that no attempt appears to have been made to discover this acid, its non-formation is inferred from the gas which remained after exposure to water burning with the same coloured flame as carbonic oxide, whence it is concluded to have been this gas, with the intermixture of nitrogen from the ammonia, its volume being admitted to be different from that of the carbonic oxide employed; and this negative result, open to such obvious fallacies, is placed in opposition to the positive production of carbonic acid in my experiments. Lastly, after all these experiments made without carbonic acid being obtained, and after the labour bestowed in endeavouring to prove experiments in which it was produced inaccurate, or to account otherwife for their results, a gas is discovered (the discovery of it is mentioned in a note, it having been made after Mr. J. Davy's paper was written) which it is admitted is capable of converting carbonic oxide into carbonic acid, and which is procured from the same materials as oximuriatic acid, and by a process apparently not much different from that which is usually followed.

Action of hidrogen, carbonic oxide, and

There remains one point to which it is necessary to give some consideration. Mr. Davy gives a reason why, as he supposes,

supposes, the presence of hidrogen cannot, even on the hypothesis I maintain, favour the conversion of carbonic oxide into carbonic acid by oximuriatic acid. In submitting these three gasses to mutual action, the hidrogen, in combining with oxigen from the oximuriatic, can form no more water than is required to the constitution of that portion of muriatic acid with which this oxigen had been combined, and hence there is no superfluous water to be afforded to that portion of muriatic acid, which remains to be formed in the oxigenation of the carbonic oxide by the remaining oximuriatic acid. "It is impossible therefore," says Mr. J. Davy (in the style which he permits himself to use) "that the latter portion of muriatic acid can become gaseous; ...... yet it does become, according to the experiment, muriatic acid gas, which is a contradiction, and by itself a proof of the innacuracy of the hypothesis."

oximuriatic acid.

This objection had occurred to me, and I had stated it as well as the solution of it to some of my friends. Of course however, a difficulty of this nature could not be urged against the fact, that the conversion of carbonic oxide into carbonic acid, which does not happen when oximuriatic gas alone is added to the former gas, takes place when there is an addition of hidrogen. And as farther the difficulty appeared to me capable of being solved, I thought it unnecessary to add to the length of my paper by taking notice of it. It is explained, I conceive, in the following manner. Muriatic acid gas may be formed with a less proportion of water than is necessary to its full saturation; the proof of its existence with an inferior proportion of water to what it contains at its first disengagement in the usual process for obtaining it I have already stated, and there is nothing improbable in the supposition, that it may exist gaseous with even less water than can be inferred from that proof. And in all elastic fluids small portions of water I believe exist, of which it is probably impossible to deprive them entirely. When a mixture, therefore, of carbonic oxide, hidrogen, and oximuriatic acid gasses is submitted to mutual action, the hidrogen attracting oxigen from the oximuriatic acid, and forming with it a portion of water, this water may favour the production of a larger quantity of muriatic acid, than the quantity merely with which that oxigen had been combined, if any

Explanation of the difficulty.

any powerful affinity is at the same time exerted, having a tendency to produce that acid: now such an affinity is actually exerted by the carbonic oxide, hence it is enabled to operate with effect, an additional portion of oximuriatic acid is decomposed, and by the communication of oxigen portions of muriatic and carbonic acids are formed. This is farther aided by the portion of water which must have been contained in the portions of hidrogen, carbonic oxide, and oximuriatic gasses, suffering these changes, even though they had been previously rendered as dry as possible. Of itself, this would not be sufficient to favour the decomposition of the oximuriatic acid, but added to the quantity formed by the oxigenation of the hidrogen it may be adequate to this effect, to the extent I have stated. The operation of this small quantity of water, remaining in elastic fluids in the driest state to which they can be brought, is very well shown in the results of the detonation of hidrogen and oximuriatic gasses, as related by Mr. H. Davy. Mr. J. Davy, indeed tells us, that he has seen this experiment made a number of times, "and though correctly made, there is no condensation attending their union." But his brother informs us, that, when the gasses are mixed over water, introduced into an exhausted vessel, and fired by the electric spark, there is a condensation of from $\frac{1}{10}$ to $\frac{1}{20}$ of the volume. I have attempted, he continues, "to make the experiment in a manner still more refined, by drying the oximuriatic acid, and the hidrogen, by introducing them into vessels containing muriate of lime, and by suffering them to combine at common temperatures, but I have never been able to avoid a slight condensation, though in proportion as the gasses were free from oxigen or water, this condensation diminished." This condensation must be owing to the water remaining in the gases even after the action of muriate of lime; and it is this, no doubt, which partly operates in the experiments I have related, and favours the result.

Conversion of carbonic oxide into acid by oximuriatic gas with a little water.

Without any reference to this experiment I would farther observe, that the conversion of carbonic oxide into carbonic acid, when oximuriatic acid acts upon it with the admission of a little water, is conclusive against Mr. Davy's hypothesis. The only mode in which the result can be accounted

counted for, in conformity to that hypothesis, is to suppose the water to be decomposed. This I had anticipated, and had observed, that of this supposition we have not only no proof, but we have sufficient reason from known facts to reject it. "Water is not decomposed by oximuriatic acid gas, or by carbonic oxide gas; there is no reason to conclude, that its decomposition can be effected by their action, when they are presented to it merely in a mixed state; and the more obvious operation may be regarded as the real one, that it acts by its affinity to muriatic acid."

In answer to this, Mr. J. Davy remarks, have we not the most indubitable evidences of the decomposition of water by oximuriatic gas? "Pass this gas and steam together through a tube heated to redness, oxigen gas will be produced, and muriatic acid gas formed. Detonate this gas and hidrogen together, and a similar formation of muriatic gas will take place. With these facts in view, who can hesitate in asserting, that water is decomposed by oximuriatic gas, that its hidrogen is attracted by this substance, and its oxigen consequently set free."

Detonation of oximuriatic gas with hidrogen.

The decomposition of oximuriatic gas by detonation with hidrogen gas, or its combination with that gas, according to Mr. Davy's view, affords no proof of the power of water to decompose it. Admitting the decomposition of oximuriatic gas by water at ignition, and supposing that we know nothing as to the mutual action of these substances at low temperatures, the conclusion would not follow, that at such temperatures this decomposition would take place. But to draw this conclusion in the very face of the fact itself is, I confess, a mode of reasoning altogether new to me. We know, that oximuriatic acid is not decomposed by water at natural temperatures; and knowing this I have no hesitation in assserting, notwithstanding Mr. Davy's indubitable evidences, that the supposition of water being decomposed in the slow mutual action of carbonic oxide and oximuriatic gasses is altogether gratuitous, and made to avoid a difficulty, which cannot otherwise be explained.

Conclusion.

And lastly let it be remarked, that putting aside all these experiments the two hypotheses still remain at least on equal grounds. The experiments were supposed to afford

afford evidence in favour of the common doctrine of the nature of the relation between muriatic and oximuriatic acids, and were they proved to be inconclusive, it would only stand in the same rank with the opinion advanced by Mr. Davy; it can be applied, as I trust I have shown in my former paper, to the explanation of all the phenomena, and to some of them with more advantage than the other; and it has besides, as an hypothesis, that superiority from general considerations, which I have also stated to belong to it.

I am, with much respect,

Your most obedient servant,

JOHN MURRAY.

IX.

On the Production of Hyperoximuriate of Potash, considered with respect to Mr. DAVY'S *Ideas of the Nature of Oximuriatic Acid. In a Letter from a Correspondent.*

To Mr. NICHOLSON.

SIR,

Mr. Davy's theory of oximuriatic acid applied to the hyperoximuriate of potash.

I Beg leave to trouble you with a few observations concerning the new theory of the oximuriatic acid as advanced by Mr. Davy, more especially as to its application to the phenomena attending the production of the so called hyperoximuriate of potash. If you think them worthy of a place in your valuable Journal, I shall consider myself honoured by their insertion. To Mr, Davy I shall offer no apology for the freedom, with which I have treated the subject; as I am convinced, that that distinguished gentlemen is more anxious for the establishment of truth, than the introduction of a favourite hypothesis.

Peculiarity of his theory.

The peculiarity of Mr. Davy's theory consists in considering the oximuriatic acid as a simple substance, a peculiar acidifying principle of a nature something analogous to oxigen. Muriatic acid he supposes to be a combination of this principle with hidrogen. He appears to me not quite to

to have made up his mind, whether this new compound enters, like other acids, into union with the different bases, or whether in the act of such union the hidrogen is again abstracted: for in one place he states, "that the compounds, "which have been usually considered as *muriates*, or as "dry muriates, are properly combinations of *oximuriatic* "acid with inflammakle *bases**," thus setting aside any union with the muriatic acid: and in another he asserts, that "muriate of ammonia is a compound of *muriatic* acid "*gas* with ammonia"†.

Formation of hyperoximuriate of potash.

The process for forming the hyperoximuriate of potash, to which I purpose to confine my observations, is this.—Black oxide of manganese is put into a retort connected with a Woulfe's apparatus, into the bottles of which a solution of potash is poured. Muriatic acid is added to the manganese, and heat applied.

Oximuriatic acid passes over and combines with the potash; and after the operation is completed, *two* distinct salts are found in the bottles, viz. *hyperoximuriate*, and common *muriate* of potash.

The new theory accounts for the production of oximuriatic acid:

The muriatic acid is of course the body, from which the oximuriatic is to be derived, by some change operated in it by the oxide. The new theory says, that muriatic acid is a combination of oximuriatic with hidrogen. The oxigen then of the manganese combines, from superior affinity, with this hidrogen to form water, and thus liberates the oximuatic acid. So far the application might be deemed satisfactory.

but how does it apply to the formation of the hyperoximuriate of potash?

Let us now proceed to account on the same principles for the phenomena which occur, when this peculiar acidifying principle comes into contact with the solution of potash. There are two ways in which this may be attempted.—We must suppose, as Mr. Davy seems inclined to do, that *muriate* of potash is a combination of *oximuriatic* acid with *metallic potassium*, or that it is a compound of *muriatic* acid with *potash*.

Difficulties in the 1st. supposition.

Let it be granted, that it is a combination of oximuriatic acid and potassium. When the oximuriatic acid (the formation

* Phil. Trans. 1810: or Journal, vol. XXVII, p. 327.

† Ibid. p. 328.

ation of which we have accounted for) comes into contact with the solution of *oxide* of *potassium*, we must suppose, that part of it from superior affinity displaces *part* of the oxigen, and combines with the potassium. But how shall we in the first place account for this partial action? If a superior affinity exist between *part* of the oximuriatic acid and *part* of the potassium, how is it that it does not subsist between the whole? How is it, that the whole oxigen of the potash is not set free, and the combination consist of muriate of potash only?

But what becomes of that portion of oxigen, which is thus liberated? Does it unite with the remainder of the oximuriatic acid? and so united, do they combine with the remaining oxide of potassium? Or is it attracted by the already saturated *oxide*, and that too in the face of a superior affinity?—This is establishing a new law of affinity at once. We assert, that *part* of the oximuriatic acid will not combine with the oxide of potassium, but overcomes a powerful* affinity to form a separate combination; and, that another part not only does unite with the same oxide, but solidifies along with it that portion of oxigen, with which the first part was incapable of uniting. To anomalies, or, more correctly speaking, to contradictions like these, it would be difficult to bring the mind to assent, even if there were no other means of accounting for the changes in the process.

Difficulties on the 2d.

Let us now examine the second supposition, under which this new theory may be attempted to be applied, viz. that muriate of potash is a compound of muriatic acid and potash. We must now suppose, that, when the oximuriatic acid first enters the solution of potash, part of it abstracts from the water of the solution a portion of hidrogen; and, being thus changed to muriatic acid, combines with the potash to form muriate of potash. The oxigen thus liberated unites to the other portion of the oximuriatic acid, and forms with the remainder of the potash the hyperoximuriate.

Now, setting aside the anomaly of a simple substance being capable of uniting either with oxigen or hidrogen to

* Mr. Davy states it to be an intense affinity. See as above, p. 330.

form

form with each of them an acid compound, (a difficulty of which Mr. Davy is aware) observe to what an insurmountable inconsistency this leads. We first of all account for the decomposition of the muriatic acid in the retort by an affinity between oxigen and hidrogen superior to that between hidrogen and oximuriatic acid, and we now account for the formation of the salts by an affinity between hidrogin and oximuriatic acid superior to that between hidrogen and oxigen. This is a complete contradiction in terms, and of course the hypothesis is untenable for a moment.

This inconsistency, permit me to remark, appears upon the face of Mr. Davy's own memoirs. He ascribes the production of water, when muriatic acid gas is passed over litharge, to the superior affinity between *oximuriatic* acid and *lead*, and the consequent union of the hidrogen of the one and the oxigen of the other *; and he accounts for the oxide of tin and the muriate of ammonia, obtained by ammonia, upon adding water to Libavius's liquor, by a superior affinity between *oximuriatic* acid and *hidrogen* †. In the first place water is *composed* because the affinity of oximuriatic acid for a *metal* is *greater* than the quiescent affinities, taken together, of oximuriatic acid for hidrogen and the metal for oxigen; and in the second, water is *decomposed* because the affinity of *oximuriatic* acid for a *metal* is *less* than the now divellent affinities of oximuriatic acid for hidrogen and the metal for oxigen. Farther anomaly.

Now unless it can be shown, that the oximuriatic acid has an affinity for lead *very far indeed* superior to what it has for tin (which cannot be done, for the difference is on the other side, and thus adding to the difficulty) it is plain, that this is as complete a contradiction as that which we have just noticed.

I should wish to enter more fully into this subject, but I have already exceeded the limits which I had prescribed to myself. At some future time I may resume the discussion.

I trust, that what I have said may have this benefit, that it will lead to farther investigation, and thus prevent the too hasty adoption of an hypothesis, not yet sufficiently grounded upon facts and experiments, and the too incon-

* See as above, p. 326. † Ib. p. 327.

siderate rejection of a theory, beautiful on account of its unity and simplicity.

I am, sir,

Your most obedient servant, and constant reader,

F. D.

19*th March*, 1811.

X.

Observations respecting the Irritability of the Barberry, Berberis communis. By Mr. ROBERT LYALL, *Surgeon, Member of the Royal Physical Society of Edinburgh, and corresponding Member of the Literary and Philosophical Society at Manchester. Communicated by the Author.*

Irritability of the stamens of the barberry.

ALTHOUGH it was known many years ago, that the *stamina* of the barberry contracted when irritated by mechanical stimuli, yet till the time when Dr. Smith published his observations relative to the subject *, our knowledge on this point was but incomplete.

Dr. Smith's experiments on them.

Dr. Smith, in order to ascertain what part of the stamina was endued with irritability, cut off one of the petals of the flower so carefully as not to touch the stamen, that stood next it; then with a very slender piece of quill he touched the outside of the filament, which had been next the petal, but it remained perfectly immovable. With the same instrument he then touched the back of the anthera, then its top, its edges, and at last its inside; still without any effect.—But the quill being carried from the anthera down the inside of the filament, it no sooner touched that part, than the stamen sprung forwards with great vigour to the stigma. From the result of this and other experiments Dr. Smith concludes, "That the motion above described was owing to a high degree of irritability in the inside of each filament, next the germen, by which, when touched, that side becomes shorter than the other, and consequently the filament is bent towards the germen."

Manner in which he accounts for it.

* Phil. Trans. for 1786.

Bonnet's

Bonnet's opinion is quite in opposition to Dr. Smith's; for he thinks, that the movement of the stamina of the barberry so much resembles the motion of a spring which unbends itself, that it cannot be attributed to irritability: an opinion certainly that requires only the evidence of seeing the experiment performed, to refute it entirely. Would even the most elastic spring, formed like the stamina of the barberry, and like them also simply fixed by one end (as if one end of the spring were nailed to a board, and not fixed by any other mechanical contrivance) move when merely touched gently at a particular part of the fixed end? certainly not. It may be observed indeed, that, if the loose end of the spring were touched gently, a vibratory motion would be produced; but this motion is very different from the gradual and steady movement of the stamina of the barberry.—Why then employ such an hypothesis, to account for their motions? Bonnet's opinion.

Sennebier, when treating of the barberry, speaks thus: "It has not yet been proved, that the movement of the stamina is accompanied with the contraction of the filaments, which nevertheless was the first proof necessary to have been given, in order to ascertain their irritability; it is not yet even well known, which is the irritable part of the filaments, and whether it be only their base, as Smith has had the address to discover." How Sennebier came to express himself in this ambiguous manner is to me a matter of surprise.—Indeed the different parts of this sentence seem to contradict each other. Sennebier's remarks on it.

Dr. Smith, as we have seen, has stated in a very perspicuous and definite manner, that the inside of the base of the filament possesses a high degree of irritability; and his experiment, which I have quoted, shows evidently, that this part only is irritable. He has said also, that one side of the filament becomes shorter than the other. Why then is it stated by Sennebier, that we even do not well know which is the irritable part of the filaments, and whether it be only their base, *as Smith has had the address to discover?* Why does Sennebier confess, that Smith has had the address to discover, that the base of the filament only is irritable, immediately after he has asserted, that we are ignorant

rant regarding the irritable part of the filament? Certainly, Sennebier did not doubt the accuracy of Dr. Smith's observations; and had he repeated the Doctor's experiments before he wrote, I think, he would never have treated of the irritability of barberry in the manner which he has done.

Dr. Smith's experiments repeated,

Some of the above remarks were written last year, at a period, when it was not in my power to repeat Dr. Smith's experiments. This season however presenting ample opportunity for the purpose, I readily embraced it; and now it is with pleasure I state, that, on comparing my remarks with those of Dr. Smith, they perfectly coincided: and that I have no hesitation in saying, that the inside of the base of the filament is the *only* irritable part of the stamina.

and varied.

To ascertain the last mentioned point clearly, and that I might be allowed to experiment easily, I cut or pulled off the calyx and corolla from a number of flowers: and, not being careful to avoid irritation, the contraction of the stamina always took place.—In a few minutes after, however, the stamina completely expanded: and then I repeated Dr. Smith's experiments, as well as others, sometimes without detaching the flowers from the bush, at other times with flowers which I had carried to my room.

The motion of the stamens takes place under water.

The motions of the stamina, I observed, were performed with as much vigour under water, as when the flowers were growing on the bush.

They will contract a 2d time in a few minutes.

I likewise ascertained, that the stamina, after being once contracted, were capable, in healthy flowers, of undergoing a second contraction, after an interval of five minutes, or sometimes in a much shorter period.

At a future time, I intend publishing the result of my experiments on the barberry with chemical stimuli.

X.

XI.

Extract of a Letter from the Rev. JOHN BRINKLEY, *D. D. F. R. S. Andrews Professor of Astronomy in the University of Dublin, to the Rev.* NEVIL MASKELYNE, *D.D. F. R. S. Astronomer Royal, on the annual Parallax of α Lyræ.*

I HAVE now had sufficient experience of my eight feet circle, to be highly satisfied with it, and have arrived at one conclusion, that it is of importance in astronomy.

My observations on α Lyræ for the purpose of discovering an annual parallax now amount to 47 in number, viz. 22 near opposition, and 25 near conjunction, and the mean of these gives a result of 2·52″ as the parallax of the annual orbit for that star; and I have no doubt, that it exceeds 2″. Annual parallax of α Lyræ.

My observations of different circumpolar stars, and of the same star in different states of the thermometer, seem to require a small alteration in the numbers of Dr. Bradley's formula for refraction. Dr. Bradley's formula for refraction

The formula so altered is—Refraction $= 56{\cdot}9'' \times$ altered

$$\text{tang.} \left\{ \text{Zen. dis.} - 3{\cdot}2 \text{ Refr.} \right\} \times \frac{\text{height of barom.}}{29{\cdot}6} \times \frac{500}{450 + \text{ther.}}$$

By means of this formula, the observations of circumpolar stars considerably distant give the same colatitude to a great degree of exactness.

SCIENTIFIC NEWS.

AT the meeting of the Wernerian Natural History Society, at Edinburgh, on the 12th of January last, Professor Jameson read the first part of a series of observations on the geognostic relations of the rocks in the island of Arran. In this memoir he described particularly the granite, gneiss, mica-slate, and clay-slate formations, and also the red sandstone and porphyry-slate, which occur so abundantly in that island. When describing the granite, he stated, as a conjecture

Wernerian society.

Geognosy of Arran.

Quartz perhaps older than granite.

jecture, that quartz might prove to be an older formation than granite, because the oldest granite contains much quartz, but little mica, and less feldspar than the newer varieties. He pointed out several observations to be made, with the view of verifying or of refuting this conjecture. In his description of gneiss, he alluded to the veins of granitic gneiss which traverse it, and which, when the gneiss and granite are in contact, have been represented as veins of granite shooting from the subjacent into the superincumbent rocks. The red sandstone the professor appeared inclined to refer to the first or old red sandstone of Werner. When describing its stratification and structure, he pointed out the appearances that ought to be attended to in endeavouring to ascertain the dip and direction of strata, and particularly cautioned observers against confounding the structure of individual strata or beds with the direction and dip of the strata. The numerous fissures that traverse the sandstone of Arran, and which exhibit every variety of magnitude, direction and dip, afforded apt illustrations of Werner's theory of veins. The porphyry-slate the Professor described as appearing in the form of overlying conical, or irregular tabular-shaped masses, resting on the red sandstone, also in veins traversing granite, sandstone, greenstone, and other rocks. He gave a description of some tabular masses of this rock, accompanied with pitchstone and claystone, contained between strata of sandstone, and which might be confounded with beds; but which he was inclined to consider merely as lateral branches of veins, or as slightly inclined veins.

Direction and dip of strata.

Geognosy of Arran continued.

At the next meeting, on the 2d of February, Professor Jameson read the continuation of his mineralogical observations on Arran. He first detailed the geognostic relations of the floetz greenstone of that island. From this account it appeared to occur in overlying masses resting on sandstone, in beds in sandstone, and in veins that traverse sandstone and other rocks. He next described the various kinds of pitchstone that occur in Arran, and seemed to think one of the varieties was so well marked, that it might constitute a distinct subspecies of the pitchstone. The account of its geognostic relations afforded a detail of many curious

geognostic

geognostic appearances, in particular the structure of its veins, and the nature of the interposed tabular masses, having many of the characters of beds, yet appearing to be either nearly horizontal veins, or lateral branches of common veins. The claystone of Arran, which was next described, appeared to occur in overlying masses along with the porphyry-slate, and also in veins along with pitchstone and porphyry-slate. It would seem that wacke and basalt are not very frequent or abundant rocks in Arran; but when they are observed, they present the usual appearances and geognostic relations.

From the observations in these two memoirs it appears, that this island contains no transition rocks; but is principally composed of primitive and floetz rocks. The alluvial rocks that occur in the valleys present the usual characters of the rocks of this class.

Mr. Singer informs me, that he has prepared some novel apparatus for the application of common electrical powers to the purpose of chemical analysis, for the illustration of his lectures on the chemical agencies of electricity, the first of which will be given on thursday, the 11th of april. His galvanic battery exposes upward of 14000 square inches of zinc surface.

TO CORRESPONDENTS.

Mr. GOWAR's paper was too late for insertion in the present number.

ERRATUM.

Page 197, *line* 22, *for* hypothesis; for the, *read* hypothesis. Farther,

METEOROLOGICAL

METEOROLOGICAL JOURNAL,

For MARCH, 1811.

Kept by ROBERT BANCKS, Mathematical Instrument Maker, in the STRAND, LONDON.

Day of FEB.	THERMOMETER. 9 A. M.	9 P. M.	Highest in the Day	Lowest in the Night.	BAROMETER, 9 A. M.	RAIN, noted at 9 A. M.	WEATHER. Day.	Night.
27	44°	44°	48	40°	29·31		Cloudy	Fair
28	47	47·5	51	40	·42	·030	Rain	Cloudy
MAR.								
1	43	42	46	38	·61	·075	Fair	Fair
2	47	48	51·5	46	·82		Ditto	Fair†
3	48·5	47	53·5	45	·80	·010	Ditto	Ditto
4	49	49	54	46	30·02		Ditto	Ditto
5	47	50	51·5	43	29·74	·005	Ditto	Rain‡
6	44	50	51	48·5	·48	·080	Ditto	Ditto
7	51	49	52·5	47	·38	·215	Rain	Rain
8	49	43	49	36	·35	·195	Ditto	Cloudy
9	38	40	43·5	37	30·26	·110	Fair	Fair
10	42	46	50	42·5	·47		Ditto	Ditto
11	44	47	50	44·5	·46		Ditto	Foggy
12	47	47	52	40	·43		Ditto	Cloudy
13	44	45	51	41	·38		Ditto	Ditto
14	44	44	47	38	·34		Ditto	Ditto
15	41	42	48	36·5	·35		Ditto	Fair
16	40·5	44	49·5	37	.29		Ditto	Ditto
17	41	44	50	37	·16		Ditto*	Ditto*
18	40·5	45	55	42·5	·14		Ditto*	Ditto
19	48	47	55	43·5	.18		Ditto	Ditto
20	49	50·5	53	49	.15		Cloudy	Cloudy §
21	51	52·5	56·5	47	.03	·040	Ditto	Rain
22	49	44	49	37	·15	·020	Fair	Fair
23	42	45	52	38	·50		Ditto	Ditto
24	46	45·5	54	40	·37		Ditto	Ditto
25	46	44	52	39	·18		Ditto	Ditto
26	46	45·5	55	38	·07		Ditto	Ditto
27	43	45	54	36	·30		Ditto	Ditto
28	39	46	51·5	40	·47		Ditto	Ditto

·780 Inch. since last Journ.

* Intervening Fogs. † Cloudy and boisterous after 11. § Rain in the Night.

‡ The early part of the Evening remarkably fine, giving me an opportunity of observing Jupiter, with a four-feet reflector, with a power of 400.

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

SUPPLEMENT TO VOL. XXVIII.

ARTICLE I.

On the Gaseous Combinations of Hidrogen and Carbon. *By* THOMAS THOMSON, *M. D. F. R. S. E.**

THE gasses formerly distinguished by the name of *heavy inflammable airs*, which are evolved during the distillation of most animal and vegetable substances, differ so much from each other in their properties, that it has been hitherto impossible to reduce them under a small number of species. When burnt, they all yield carbonic acid and water, and of course contain carbon and hidrogen; but the proportion of these bodies formed, and of oxigen consumed during the combustion, varies very much at different times. Many of these gasses have been examined with much address by Cruickshank, Dalton, and Henry. Berthollet also has examined them at different times, and published very ingenious dissertations on their composition. Gaseous compounds of hidrogen and carbon.

From a great many experiments on these gasses, at different times, and in different states, I have convinced myself, that they usually hold an empyreumatic oil in solution; and that the differences in their specific gravity, and other properties, depend very much upon the proportion of oil Their differences depend much on the presence of oil,

* From the Wernerian Transactions.

present. Hence no pure gasses, fit for examination, and comparable with each other, can be obtained from those vegetable or animal substances which yield an oil when subjected to heat, as is the case with most of them. To this oil is to be ascribed the great variation in the specific gravity of the gas obtained by distilling pit-coal, as shown by the experiments of Dr. Henry.

and carbonic oxide.

When water and carbon are present together, as is the case in most animal and vegetable substances, they act on each other, and give origin to variable quantities of carbonic oxide, which must also very much alter the properties of the gas evolved. To these two causes, namely, the oil dissolved, and the carbonic oxide formed, most, if not all the varieties in the combustible gasses, obtained from animal and vegetable substances, are owing.

Supposed to contain oxigen.

Berthollet, in a dissertation which he lately published, in the second volume of the Mémoires d'Arcueil, has endeavoured to prove, that all the heavy inflammable gasses contain oxigen as a constituent. He has examined a number of gasses obtained by distilling charcoal, and has shown, that each of them contained a considerable proportion of oxigen. This opinion has likewise been maintained by Mr. Murray in his System of Chemistry.

If any confidence can be put in the preceding observations, it is clear, that this obscure subject can never be elucidated by examining gasses from charcoal, or from animal and vegetable substances. The first will always yield carbonic oxide as well as carburetted hidrogen, and the gas from the other bodies will be disguised by the oil held by it in solution.

A pure carburetted hidrogen probably exists.

Analogy is strongly in favour of the common opinion, that there exists a gas composed simply of hidrogen and carbon. Hidrogen, we know, is capable of dissolving every other simple combustible, sulphur, phosphorus, and boracium. It dissolves likewise several of the metals, as arsenic, zinc, tellurium, and potassium. Why then may it not be capable of dissolving carbon?

Gas from stagnant water examined.

There is a gas which rises in considerable quantity from stagnant waters during the summer season. It was examined by Cruickshank and Dalton, and both of them concluded

cluded it to be a compound of carbon and hidrogen, without any sensible quantity of oxigen. But as neither of these gentlemen has published a detailed account of his experiments, I thought it worth while to examine the gas anew, in order, if possible, to determine the point in a satisfactory manner. I accordingly collected considerable quantities of it at different times in the neighbourhood of Restalrig, where ponds of muddy water are left stagnant, in order to collect manure. This gas I found to have the following properties.

Its properties.

1. It is colourless, and transparent like air.

2. It has no smell, and no sensible taste, provided it be previously washed in clean water.

3. It always contained a mixture of carbonic acid. The least quantity of this gas present was 5 per cent; the greatest $7\frac{1}{2}$ per cent.

4. It always contained a mixture of common air. It is remarkable, that the proportion present was in every case the same, and amounted to 12·5 per cent, or 2·5 per cent of oxigen and 10 per cent of azote. Mr. Dalton says, that the gas which he examined contained 20 per cent of azote. This was never the case with the gas from Restalrig; but the common air and carbonic acid gas together sometimes amounted to 20 per cent, and always to near that quantity.

5. After depriving it of its carbonic acid, I found its specific gravity 0·611, that of air being 1·000. But as it contained a mixture of 12·5 per cent of air, it would have been obviously lighter, if this portion had been removed. By calculation I find, that the specific gravity of the pure gas would have been 0·5554 *.

Method of taking the specific gravity of gasses.

My method of taking the specific gravity of gasses being extremely simple, yet precise, I think it worth while to describe it in this place. It is founded on the well-known fact, that, when two gasses are mixed, their bulk does not

* Let $x =$ the specific gravity of the pure gas, $A =$ the quantity of air in the mixture, $a =$ the specific gravity of air, $B =$ quantity of pure gas present, $c =$ specific gravity of the mixture, we have $x = \frac{(A+B)\,c - Aa}{B}$.

alter

alter. I have a large flask fitted with a stop-cock. I weigh this flask very accurately, then exhaust it, and weigh it again. Let the loss of weight be a. I then fill the flask with the gas, the specific gravity of which I want, and weigh it again. If the weight of the flask be the same as at first, it is obvious, that the gas has exactly the specific gravity of common air. If it be heavier than at first, the gas is heavier than common air; if lighter, the gas is lighter than common air. Suppose the flask lighter than at first, I add weights till the flask is exactly poised. Let the weight added be b. We have the weight of common air to that of the gas as a to $a-b$. And to find the specific gravity, we have this proportion, $a : a-b :: 1 : x =$ specific gravity wanted, or $x = \frac{a-b}{a}$. If the flask is heavier than at first, weights are added to the other scale, till it is exactly poised; let these weights, as before, be b, we have in this case $x = \frac{a+b}{a}$. In this process no correction is necessary for temperature, or the state of the barometer; because air and all gasses undergo the same change of bulk, by changing the temperature and the pressure. The gas is always weighed saturated with moisture. This is corrected when requisite by the table of Dalton. In this way the flask may be easily weighed, true to the $\frac{1}{100}$dth part of a grain; so that you are always sure of the specific gravity of the gas, to the 3d decimal figure. The specific gravity of common air is always taken 1·000, and at the temperature of 60°, barometer at 30 inches, 100 cubic inches of it weigh 30·5 grains. These data enable us to determine the weight of 100 cubic inches of any gas the specific gravity of which is known. Mr. Dalton found the specific gravity of this gas 0·600. I took its specific gravity more than once, and collected at different times, but never found it heavier than 0·5554.

Farther properties of the gas from stagnant water.

6. The gas from stagnant water burns with a yellow flame, more readily, and with a larger flame, than any other combustible gas, except olefiant gas, and the vapour of ether.

7. When mixed with oxigen gas, and kindled by an electric spark, it detonates loudly, and undergoes a considera-

ble

ble diminution of bulk. Its combustibility by this process is confined within very narrow limits. It does not burn, unless the bulk of the oxigen rather exceeds its own bulk, and it ceases to burn, when the oxigen gas is more than $2\frac{1}{4}$ times the bulk of the inflammable gas. The limits of combustion, according to my trials, are 100 measures of inflammable gas, and 105 or 227 measures of oxigen gas. A mixture of 100 measures of inflammable gas with 282 or 292 measures of oxigen gas does not burn.

8. A good many experiments on the combustion of this gas with oxigen by electricity, give the following general result: 100 measures of the inflammable gas consume 205 measures of oxigen gas, and 104 measures of carbonic acid gas are formed. This result very nearly agrees with the experiments of Mr. Dalton, who found, as I have been informed by Dr. Henry, that 100 measures of this gas require for combustion 200 measures of oxigen gas, and form 100 measures of carbonic acid. The following table contains a few of the experiments which I made upon this gas. The others were precisely of the same kind, and coincided with these as nearly as possible.

Experiments made on this gas.

The oxigen gas used contained 12 per cent of azote. It had been prepared from hyperoximuriate of potash, and, when recent, contained only 1 per cent of azote. It had been kept above a year in a crystal bottle well stopped; but had been often opened, and portions of it used during the interval. The bottle was now half full of water. The purity of this gas was tried just at the time of making the following experiments, by exposing it to hydroguretted sulphuret of lime in the usual way. The nitrous gas employed contained, by the test of sulphate of iron, 11·5 per cent of azote. The inflammable gas had been freed from carbonic acid, but it contained 13·5 per cent of common air.

Experiments.

	Measures of inflammable gas.	Measures of oxigen.	Bulk of residue.	Bulk of do. when washed in lime-water.	Measures of nitrous gas added to do.	Bulk of residue.	Bulk of do. washed in sulphate of iron.
1	25	50	31	9	22	24	9
2	25	60	40	17	40	30	12
3	20	55	Does not burn.				
4	25	55	44	38	88	42	18
5	25	50	31	8·5	39	32	10
6	40	35	Does not burn.				
7	40	40	Burns.				

From this table, and our knowledge of the constituents of the gasses used, it is easy to deduce the following:

	Gasses examined.				Carbonic acid formed.	Residue.	Composition of Residue.	
	Pure inflammable gas.	Pure oxigen.	Azote.	Total.			Azote.	Oxigen.
1	21·6	44·7	8·7	75	22	9	8·7	0·3
2	21·6	53·5	9·9	85	23	17	9·9	7·1
3	17·3	48·9	8·8	75				
4	2·16	49·1	9·3	80	6	38	9·3	29
5	21·6	44·7	8·7	75	22·5	8·5	8·7	0
6	34·6	31·9	8·5	75				
7	34·6	36·3	9·1	80				

Of these experiments, the 1st, 2d, and 5th are the only ones, from which the proportion of oxigen consumed, and of carbonic acid formed, can be deduced. They were repeated

repeated very often, in order to insure as much accuracy as possible. The following is the result which they give:

Results.

Inflammable gas consumed.	Oxigen gas continued.	Carbonic acid gas formed.	
21·6	44·7	22	
21·6	43·6	23	
21·6	44·7	22·5	
21·6	44·3	22·5	average.
100	205	104	aver. per cent.

9. From these experiments it is easy to deduce the composition of this inflammable gas. Its specific gravity being 0·555, 100 cubic inches of it, at the temperature of 60°, and when the barometer stands at 30 inches, will weigh 16·93 grains. Its composition.

Of the 205 cubic inches of oxigen gas consumed, 104 go to the formation of carbonic acid gas; the remaining 101 cubic inches must have combined with hidrogen during the combustion, and formed water. Hidrogen, in the gaseous state, combines with exactly half its bulk of oxigen gas; therefore the hidrogen, which went to form water in the present case, must have been equivalent to 202 cubic inches.

If we suppose with Saussure, (and it is the supposition least favourable to our present purpose), that carbonic acid gas contains 27·5 per cent of carbon, in this case the carbon contained in 104 cubic inches of it must weigh 13·24 grains. The weight of 202 cubic inches of hidrogen gas is 5·17 grains. Thus we have ascertained, that 100 cubic inches of the inflammable gas from stagnant water contain of

Carbon,	13·24
Hidrogen,	5·17
Total,	18·41

But

But the weight of 100 cubic inches of the gas is only - - - - -	16·93
So that the constituents found by analysis exceed the weight of the gas - -	1·48 or almost 1.5 grain.

This is a clear proof, that the gas contains no oxigen. The carbon and hidrogen, which we have found it to contain, constitute the whole of its weight. The small excess would nearly vanish, if we were to suppose the specific gravity of the gas to be 0·600, as Dalton found it. It is obviously owing to errours in the analysis, which are unavoidable when minute quantities of gaseous bodies are measured.

10. We may conclude, then, that the gas from stagnant water is entitled to the name of carburetted hidrogen, and that it is a compound of very nearly,

Carbon - - -	72
Hidrogen - -	28
	100

Mr. Dalton considers it as composed of an atom of carbon, and two atoms of hidrogen; and this very nearly agrees with the above analysis, if we suppose an atom of carbon to weigh 4.5, and an atom of hidrogen to weigh 1; for 72 is to 28 very nearly as 4.5 to 2.

Olefiant gas. There is another gas, which is at present considered as composed of carbon and hidrogen. It was discovered by the associated Dutch chemists, and called by them Olefiant Gas, in consequence of the oily-looking substance into which it is converted, when mixed with oximuriatic acid gas. The experiments of Cruickshank, Henry, Dalton, and Berthollet, all coincide to show, that its only constituents are carbon and hidrogen. Mr. Murray, however, in his System of Chemistry, has expressed his suspicion, that oxigen is also one of its constituents. He founds his conjecture on the alleged formation of carbonic acid gas when olefiant gas is passed through a red hot tube. Even if carbonic acid were formed in this case, it would not prove the gas to contain oxigen, unless the experiment be made in such a manner, as that all common air is completely ex-

cluded; and practical chemists are sufficiently aware of the difficulty of such an exclusion, when the experiment is made in the usual way. Carbonic acid gas never makes its appearance, if the whole common air be previously removed from the tube by means of a current of hidrogen, and if care be taken to separate all the oxigen, with which the olefiant gas may be mixed, by nitrous gas. It is proper to know, that liquid sulphuret of lime has the property of dissolving olefiant gas in considerable quantity. It cannot, therefore, be used to free the gas from oxigen.

Dissolved by sulphuret of lime.

Though the well-earned celebrity of the chemists, who have analysed olefiant gas, left little doubt, that their analysis was accurate, I thought it worth while to repeat their experiments, in order to add my testimony to theirs.

Olefiant gas formed.

1. One ounce measure of alcohol, of the specific gravity 0·826, and 3 ounce measures of sulphuric acid, of the specific gravity 1·860, were mixed together, and boiled in a small retort, almost to dryness; while the gaseous products were received in the usual manner over water. The gasses obtained, (not reckoning the common air of the retort, which amounted to about 42 cubic inches), were 146·12 cubic inches of olefiant gas, and 53 cubic inches of carbonic acid gas. The proportion of carbonic acid at first was very small, but it increased as the process went on, and at the end amounted to about one half of the olefiant gas. The charry matter which remained in the retort, after being well washed and dried on a sand-bath, weighed 68 grains.

Its specific gravity.

2. A portion of this olefiant gas, deprived of its carbonic acid, but containing 16 per cent of common air, was found of the specific gravity 0·9786, that of air being 1·000. Hence the specific gravity of pure olefiant gas is 0·9745. This is heavier than it was found by the Dutch chemists, who state its specific gravity at 0·909. As far as I know the specific gravity has not been taken before in this country.

Mixed with oximuriatic acid gas.

3. Being curious to know the nature of the supposed oil, which is formed when olefiant gas is mixed with oximuriatic acid gas, I filled a large bottle with olefiant gas, and passed a current of oximuriatic acid gas into it; the wished for substance soon collected at the bottom of the phial. It possessed the following properties.

(1.) It

Properties of the substance formed.

(1.) It was a liquid of a greenish white colour, with a slight smell of oximuriatic acid, which it lost when allowed to stand a sufficient time exposed to the air. Its taste was sweet and cooling, and it made a strong, though not unpleasant, impression on the palate.

(2.) When dropped into water, it fell to the bottom, and looked like so much melted phosphorus. It continued in this state for some time if left at rest; but when the mixture was agitated, it dissolved in the water. The liquid continued colourless, acquired a sweet and cooling agreeable taste, and a slight aromatic odour. It did not affect vegetable blues, but precipitated copiously with nitrate of silver.

(3.) When dropped into alcohol, the liquid appeared milky at first, but almost immediately became transparent, a complete solution being effected.

(4.) Sulphuric ether dissolved it with great rapidity, and the solution was colourless.

(5.) It did not dissolve in oil of turpentine, but continued in small globules, which attached themselves to the bottom of the vessel.

(6.) It dissolved immediately in nitric acid, without effervescence, or any apparent change in the liquid.

(7.) When dropped into sulphuric acid, a copious effervescence took place, the smell of oximuriatic acid became evident, and the liquid remained clear.

(8.) When left in an open vessel, it evaporated completely, leaving only a green trace.

Not an oil. Resembles the pyro-acetic spirit.

From these properties, it is obvious, that the liquid in question does not belong to the class of oils. It is a substance of a nature quite peculiar, and seems to consist of the two gasses simply combined together. It has considerable resemblance, at least in taste, to the pyro-acetic spirit of Mr. Chenevix.

Detonated with oxigen.

4. When olefiant gas is mixed with thrice its bulk of oxigen gas, it detonates very loudly, when an electric spark is passed through it, and burns with a strong white flame. According to Mr. Dalton, it consumes exactly thrice its bulk of oxigen gas, and forms twice its bulk of carbonic acid. My experiments, as will appear from the following

following table, very nearly coincide with his. The gas used contained 16 per cent of common air, and the oxigen gas was mixed with 11 per cent of azote.

Results.

	Measures of olefiant gas.	Measures of oxigen gas.	Residue after combustion.	Do. washed in lime water.	Measures of nitrous gas added to do.	Residue.
1	20	59	45	10	42	44
2	20	60	45	9	23	27
3	20	57	42	8	23	27

From this table we easily deduce the following:

	Measures of Gasses mixed.				Carbonic acid formed.	Residue.	Composition of Residue.	
	Pure olefiant.	Pure oxigen.	Azote.	Total.			Azote.	Oxigen.
1	16·8	53·14	9·06	79	35	10	9·06	2·9
2	16·8	54·04	9·16	80	36	9	9·16	1·8
3	16·8	51·34	8·86	77	34	8	8·6	1·4

This table obviously furnishes us with the following results:

	Olefiant gas consumed.	Oxigen gas consumed.	Carbonic acid formed.	
1	16·8	50·24	35	
2	16·8	52·24	36	
3	16·8	49·94	34	
	16·8	50·8	35	Average.
	100	302	208	Aver. per cent.

From

Its composition.

From these experiments, it is easy to deduce the composition of olefiant gas: 100 cubic inches of it, at the temperature of 60°, and when the barometer stands at 30 inches, weigh 29·72 grains.

Of the 302 cubic inches of oxigen gas consumed, 208 went to the formation of carbonic acid. The remaining 94 cubic inches must have gone to the formation of water, and they must have combined with a quantity of hidrogen, which, if in the gaseous form, would have amounted to 188 cubic inches. Therefore, 100 cubic inches of olefiant gas are composed of the carbon in 208 cubic inches of carbonic acid, and a quantity of hidrogen equivalent to 188 cubic inches.

Now, the carbon in 208 inches of carbonic acid weighs - - -	26·98 grains.
108 inches of hidrogen gas weigh -	4·80
Total,	31·78
Weight of the olefiant gas, - -	29·72
Surplus, -	2·06

Thus the weight of the constituents found by analysis exceed that of the olefiant gas by about $\frac{1}{15}$th part;—a clear proof, that olefiant gas contains no oxigen. The science of chemistry, in its present state, admits of no stronger proof, than what we have now given.

It follows from the preceding analysis, that olefiant gas is composed of about

Carbon, - -	85
Hidrogen - -	15
	100

Mr. Dalton considers it as composed of an atom of carbon and an atom of hidrogen. This comes tolerably near to the preceding analysis, if we suppose the weight of an atom of carbon 4.5, and that of an atom of hidrogen 1. For 85 is to 15 very nearly as 4·5 is to 0·8.

Olefiant gas detonated with less than its weight of oxigen.

6. There is a curious experiment, first made by Cruickshank, and afterward repeated by Berthollet, which I thought it worth while to verify. When olefiant gas is mixed with less than its bulk of oxigen, and the mixture is

fired

fired by electricity, a quantity of charcoal precipitates, and the bulk of the residue after the detonation is much greater than before. The following table exhibits the result of my experiments in this way:

	Measures of olefiant gas.	Measures of oxigen.	Residue after combustion.	Do. washed in lime water.	Measures of nitrous gas added.	Residue.
1	39·5	28·5	100	94·5	37	121·5
2	40	30	110			
3	40	30	110			
4	40	29	110			

In each of these experiments, a quantity of charcoal was precipitated. It remained long suspended in the gaseous residue, quite dry, and made its escape into the air, if the detonating tube was turned up. I attempted to ascertain the weight of this charcoal, but was not successful. It was necessary, in order to collect it on the filter, to moisten it in the first place; and it was found impossible to dissipate the whole of the water, without altering the filter. Hence it was always a great deal too heavy, never weighing less than $\frac{1}{5}$th of a grain. Charcoal obtained.

From the preceding table we easily deduce the following:

	Pure olefiant gas.	Pure oxigen gas.	Azotic gas.	Total.	Carbonic acid formed.	Residue.
1	33·2	26·64	8·16	68	5·5	94·5
2	33·6	27·98	8·42	70		
3	33·6	27·98	8·42	70		
4	33·6	27·08	8·32	69		

It

In the first experiment, 5.5 measures of oxigen gas went to the formation of carbonic acid, and 3·74 were found uncombined, after the explosion, by the test of nitrous gas. Hence it follows, that 17·4 measures of oxigen gas, combined with 33·2 measures of olefiant gas deprived of a portion of its carbon, and formed a new inflammable gas, amounting to 82·7 measures in bulk, or almost double the bulk of the two gasses that went to form it.

New inflammable gas formed.

This examined.

The new inflammable gas being examined by a new mixture with oxigen gas, and a new detonation, was found to consume 73 per cent of oxigen gas, and to form 55 per cent of carbonic acid.

Hence the 82·7 measures would have formed -	45·5
But 33·2 measures of olefiant gas would have formed	69·0
Difference,	23·5

From this it would seem, that about $\frac{1}{4}$th of the carbon removed by the first explosion is converted into carbonic acid, while $\frac{3}{4}$ths precipitate in the state of a black powder.

33·2 cubic inches of olefiant gas weigh	9·87 grains.
The carbon in 23·5 cubic inches of carbonic acid weighs - - - - - - - - -	3·05
Residue,	6·82
17.4 cubic inches of oxigen weigh - - -	5·31
Hencce the weight of 82·7 cubic inches of the new gas cannot exceed - - - -	12·13

Hence 100 cubic inches of it would weigh 14·66 grains, and its specific gravity cannot exceed 0·4808.

This is on the most unfavourable supposition, that no water whatever is formed during the first combustion. If water be formed, it is obviously lighter than we have made it. It is clear, therefore, that this new-formed gas, to which the name of oxicarburetted hidrogen may with propriety be given, is quite different from carbonic oxide gas, the specific gravity of which is 0·956, or almost double of our new gas.

9·87

9·87 grains of olefiant gas are composed of 1·49 hidrogen.
8·38 carbon.

9·87

Hence our oxicarburetted hidrogen gas is composed of - - - - - - - - - 1·49 hidrogen. Its composition.
5·33 carbon.
5·31 oxigen.

12·13

or per cent of	carbon	43·9
	oxigen -	43·8
	hidrogen	12·3
		100·0

It is not improbable, that this oxicarburetted hidrogen gas is composed of an atom of carbon, an atom of oxigen, and an atom of hidrogen. If this supposition be well-founded, the proportion of oxigen must exceed a little what we have obtained by our analysis. This would probably have been the case, if we had founded our analysis upon any of the succeeding experiments, rather than the first of the preceding table.

The preceding experiments, I flatter myself, entitle us to conclude, that *two* gaseous compounds of hidrogen and carbon exist. To the first we may give the usual name of carburetted hidrogen; to the second the name of supercarburetted hidrogen, as it contains very nearly twice as much carbon as the first gas does. There exists also a gaseous compound, consisting of oxigen, carbon, and hidrogen; but it differs in its properties from all other inflammable gasses hitherto examined. The reason why the inflammable gasses from vegetable and animal substances differ so much from each other is, that they usually hold an oil in solution, and are mixed with variable quantities of carbonic oxide gas. Conclusions.

II. *On*

II.

On a new Compound resulting from the Union of Benzoic Acid with Camphor. In a Letter from F. R. Gowar, *Esq.*

To Mr. NICHOLSON.

SIR,

New oleaginous compound.

SHOULD the following communication prove worthy of insertion in your Journal, it will give me great satisfaction, if I find, that I can tend in the least to the advancement of chemical knowledge, by the relation of a few experiments and observations on a new oleaginous compound.—The experiments which I particularly wish to submit to your consideration are to illustrate the formation of this substance, which I have recently produced from certain admixtures of benzoic acid, camphor, alcohol, and water; and which may also be produced in a similar manner from the sulphuric and nitric æthers, when substituted for alcohol.

Solution of benzoic acid and camphor forms an oleaginous compound, on the addition of water in a certain proportion.

By making solutions of benzoic acid and camphor in either of these solvents, I found, that, on the addition of a definite quantity of water, no precipitation of the acid or camphor was produced, but an oleaginous compound was formed. This seems to present an anomaly inconsistent with analogy; inasmuch as it proves, that water does not act primarily in causing a precipitation of these bodies, either from alcohol or æthers, a circumstance, which a priori would not have been expected; but causes rather the formation of a triple compound, the production of which is increased in the inverse ratio to the purity of the alcohol employed.—From this preliminary sketch, it appears, that water becomes necessary in the ratio of the purity of the alcohol; or to speak generally, that any given quantity of common spirit of wine would require one part, while alcohol of the sepcific gravity of 0·800 would require double to effect the same end.

This compound a benzoate of camphor,

The ternary compound, however, resulting from any one of these fluids, viz. alcohol or æthers, consists of benzoic acid and camphor, which are brought into union, and preserved

served in this state, through the medium of either of the above-mentioned solvents: therefore the union of these two bodies furnishes a very good illustration of the old chemical law:—" *Corpora non agunt, nisi sint soluta.*" But, however, this axiom has now become obsolete, and rendered totally inconsistent as to its general application, from our present and more refined laws of chemical affinity; yet, nevertheless, I think it may be considered in the present combination as one fully applicable, because it illustrates the formation of the compound. Indeed, I shall have occasion to show, by the following experiment, that, if the connecting medium or solvent of these two bodies be destroyed by increasing the quantity of water, a subsequent diminution of power of that medium as a solvent must inevitably result; and the benzoic acid will be precipitated in crystals, while the camphor, the remaining constituent, will rise to the surface of the fluid.

which will not form if the water be in too large proportion.

Experiment 1.—Twenty grains of benzoic acid, and the same quantity of camphor, were mixed in half an ounce of alcohol, which readily dissolved them without the least change; but immediately on the addition of the same quantity of water, the substance in question was produced, accompanied with a sensible evolution of caloric. This production of heat, however, must be well known to depend on the powerful attraction, which the alcohol and water mutually exert to enter into chemical combination; and may be said to depend, according to Berthollet, on the condensation of volume*.

Exp. 1.—Benzoic acid and camphor dissolved in alcohol, and water added.

On allowing this solution to remain quiet, it gradually separated into two strata; the uppermost of which was a spirituous solution of a pale yellow colour: and the lower one was the new combination.

The fluid separated into two strata.

The properties of the upper stratum are as follows: It was of a pale yellow colour: (owing, no doubt, to a quantity of resin, which the acid contained, for if the acid be perfectly pure, both the oleaginous compound, and the solution above it, are perfectly transparent.) It was acid, and when exposed to a gentle heat in a retort, a spirituous solution, containing a quantity of camphor, came over,

Properties of the upper.

* Berthollet, Chem. Stat. v. i.

which was separated in small flakes on the addition of water; while the acid remained in the retort, and precipitated on cooling in a crystallized state.

Properties of the lower.

Properties of the lower stratum, or oleaginous compound.—It was of a bright yellow colour, depending, as I have before remarked, on a quantity of resin, which the acid contained. It is strongly acid, and has a pleasant aromatic smell. When a few drops were put upon a spatula, and exposed to the heat of about 250°, it was rapidly volatalized; and on taking it away, the remaining portion crystalized upon the spatula, like rays diverging from a centre. These crystals were dissolved in alcohol, which then reddened vegetable blues. When this peculiar compound was immersed in cold water, it was immediately decomposed, the alcohol entered into chemical combination with the water; the benzoic acid was precipitated in small shining crystals, and the camphor remained on the surface of the fluid. This then is sufficiently evident, to elucidate that water, although necessary to its formation, will not preserve it when formed, without the necessary admixture of a definite quantity of alcohol.

Boiling water readily dissolves it, and precipitates on cooling about $\frac{19}{20}$ths of the acid, in small flaky crystals; while the camphor is principally separated, and rises to the surface of the fluid, leaving, however, a small portion in solution. It is very soluble in alcohol, and, if dissolved in sufficient quantity, may be recondensed into the original compound. When mixed with a solution of the subcarbonate of potash it was decomposed, a benzoate of potash was formed, and a quantity of carbonic acid gas was evolved; while the camphor, the other constituent, was liberated, and floated on the surface of the fluid. In order, therefore, to obtain a quantity of the benzoic acid in a pure state, the camphor was separated from this solution by passing it through a filter, and obtaining the solution of benzoate of potash in the receiver. To a portion of the solution thus collected, sulphuric acid was added to saturation, which, from its greater affinity, formed a sulphate of potash, to the exclusion of the benzoic acid, both of which, however, were mechanically combined, and formed a white precipitate:

precipitate: this precipitate was afterwards thrown on a filter, and washed with a weak solution of alcohol in water. After passing through the filter, and remaining some time, crystals of the acid were formed. When to another portion of the same solution (benzoate of potash) heated, sulphuric acid was added, a very peculiar change ensued, viz. the benzoic acid was immediately precipitated in beautiful flaky crystals, and left the sulphate of potash in solution.

Exp. 2.—None of the compound produced when too much of the menstruum is present.

Exper. 2.—In trying how much benzoic acid and camphor was necessary to the formation of this compound, I found, that from the following proportions none of the oleaginous substance was produced.

Benzoic acid gr. xx, camphor gr. xx, alcohol an ounce, water the same. It will appear from this experiment, that the combination is only produced, when the substances which are necessary to its formation are in a certain ratio with respect to the quantity and purity of the alcohol employed; for it seems, that when they are so much diluted with alcohol and water, the affinity of the benzoic acid for the camphor is counteracted by the superior attraction of the alcohol for this body; and thus the benzoic acid and camphor are preserved in solution, without being able to exert their mutual affinities to produce the new combination.

Crystals formed.

In repeating these experiments in larger proportions, I observed some crystals, varying from half an inch to an inch in length, were formed (on standing for several hours) in the fluid above the oleaginous compound, and which I thought, from their magnitude, were composed of benzoic acid and camphor. It may also be proper to remark, that crystals are formed in the oleaginous substance, but much inferior in their magnitude to those which are found in the supernatant stratum.

Their properties.

Examination and properties of the crystals.—They were of an irregular, oblong, and flat shape; acid, partially soluble in cold water, but perfectly so at the temperature of 212°. When dissolved in a solution of the subcarbonate of potash, a quantity of carbonic acid gas was evolved; but no camphor was separated. When dissolved in alcohol, they gave to it acid properties; and on the addition of wa-

 ter,

ter, a small quantity of benzoic acid was separated. This seemed a second confirmation, that camphor did not enter into their composition. From this examination, however, I think it is extremely probable; especially since we find the larger crystals are formed in the upper stratum, that camphor does enter into their composition; but that their increased magnitude is owing to the reciprocal union of the alcohol and water in combination with the benzoic acid.

From the two above-mentioned experiments we learn, that water is only essential to the production of the compound, inasmuch as it combines in definite ratios with the alcohol, and therefore acts as a condensing agent, whereby the particles of the acid and camphor are brought nearer together: while they in their nascent state (if I may be allowed the expression,) preserve their attractive quantities of alcohol, by which they are enabled, from their natural affinities, to enter into absolute union, and thus effect the formation of the oleaginous compound; or what may be more properly denominated a benzoate of camphor and alcohol.

It must be sufficiently obvious, from what has been above stated, that alcohol acts a very material part in the production of this compound; I therefore was induced to try, from their analogous power as solvents for these bodies, the effects of the sulphuric and nitric ethers.

Exp. 3.—Sulphuric ether employed instead of alcohol.

Exper. 3.—Twenty grains of benzoic acid, and the same quantity of camphor, were dissolved in half an ounce of sulphuric ether. To this solution the same proportion of distilled water was added, which produced at first a general turbidness; but the æther, from its not possessing much affinity for water, together with its superior levity, soon separated, and occupied the upper stratum, which on agitation had an oleaginous appearance. The result of this combination, however, seems nothing more than a separation of the ether by the mechanical admixture of water; the oleaginous appearance produced on agitation being caused by the mutual and equal repulsion of their particles, which thus gives the spherical appearance to the compound. The density of this compound may be increased by augmenting the proportions of benzoic acid and camphor. Its principal

pal properties are its specific levity, the retention of its oleaginous appearance in hot water, and finally its total decomposition in cold water.

Exper. 4.—The same proportions of benzoic acid and camphor were dissolved in half an ounce of nitric ether: the colour in this combination, however, soon changed to a reddish brown, and a quantity of an oleaginous substance of the same colour was produced, without the admixture of any water. It is probable, I think, that the colour in this combination was produced from the action of the ether on a quantity of resinous matter in the acid. From a subsequent experiment, however, precisely in the same proportion, there was scarcely any of this compound produced; but on the addition of half an ounce of water there was a copious production of it, which gravitated to the bottom of the phial. After standing for several hours, a number of crystals had formed within it, and it had become much denser, and resembled in appearance melted phosphorus. Thus we see that water greatly facilitates the formation of the compound, and also causes by dilution the subsequent formation of crystals. When these two experiments were repeated with a purer acid, in the same proportions, analogous results were produced; but the compounds were of a pale white colour: and thus, I think, proving satisfactorily, that the colour in the former experiments was owing to the impurity of the acid. The general properties of this compound are its greater specific gravity; its spontaneous formation without water, and its immediate production on the admixture of this fluid; together with the resulting formation of crystals, which I suppose are produced in the ratio to the power of dilution. It is acid, and also preserves its oleaginous appearance in either hot or cold water, and is partly dissolved. If, however, it be put into too large a quantity of either, it is extremely probable that it will be dissolved or decomposed; because the power of attraction, which will preserve it in a small quantity, will be totally annihilated by a greater.

Exp. 4.—Nitric ether.

Thus after observing the peculiar combinations of benzoic acid and camphor with alcohol and ethers, I was induced further

farther to make a series of experiments on the different vegetable, animal, and mineral acids, in order to observe whether any other acid or acids were capable of effecting a similar combination: as there seemed from analogy just grounds to suppose, that any other mild vegetable acid would produce an analogous compound, when treated in a similar manner.

Experiments.

Experiments with camphor and other acids instead of the benzoic.

5. Oxalic acid gr. xx, camphor gr. xx, alcohol ℥ss, water ℥ss.
6. Tartaric acid - - - - - - -
7. Boracic acid - - - - - - -
8. Citric acid - - - - - - - -
9. Camphoric acid - - - - - - -
10. Succinic acid - - - - - - -
11. Alcohol ℥ss, camph. gr. xx, Lactic acid ℥ss, water ʒii.
12. - - - Formic acid ʒii - -
13. - - - Acetic acid - - -
14. - - - Prussic acid - - -
15. - - - Phosphoric acid - -
16. - - - Muriatic acid - -
17. - - - Sulphuric acid - -
18. - - - Nitric acid * - - -

None of them produced such a compound.

From all these experiments, however, not one of the acids was capable of producing the substance in question; but on the contrary caused in some instances the precipitation of the camphor previous to the addition of water. Benzoic acid, therefore, may be considered as exerting a very peculiar attraction to enter into combination with camphor, when assisted by the cooperative power of alcohol or ether. It is extremely probable, that the incapability of the above mentioned acids of forming compounds with camphor may arise in general from their being stronger acids,

* Although nitric acid in this combination was incapable of producing an oily compound, yet in its undiluted state it readily dissolves camphor, forming that substance which has been improperly termed the oil of camphor, and which in my humble opinion better deserves the appellation of nitrate of camphor; since it would do away the ambiguous and unscientific term of the former, and convey, I trust, a better expression of its composition.

so as from their superior acid property totally to destroy that attraction which the alcohol exerts for the camphor; and thus, by altering the properties of the alcohol, destroy the effect of combination.

The readiness with which the oleaginous substance may be formed when benzoic acid is used may probably depend on the three following causes. 1st, That the benzoic acid naturally possesses a strong attraction to combine with camphor. 2ndly, That its attraction for alcohol is greater than that of any other acid. And 3dly, Through the medium of the alcohol, it is brought into actual combination with the camphor, so as to produce the oleaginous compound, or benzoate of camphor and alcohol. From these three causes I shall offer the following theory of its formation. As water is the principal agent in the production of this compound, it becomes necessary to consider its specific action in producing it; and first to consider its power as a solvent.— It must be well known, that water can only dissolve about $\frac{1}{20}$ part of benzoic acid; and from some recent experiments, which I have lately made on the solubility of camphor in distilled water, I find, that only a grain of this substance is soluble in two ounces, or 1624 troy grains; from which I infer its very slight power as a solvent for either of these bodies. It becomes, therefore, extremely probable, that the water exerts almost the whole of its affinity to enter into chemical combination with the alcohol, which we know from dilution would render it less capable of holding these bodies in solution. Thus it is reasonable to expect, that a quantity of benzoic acid and camphor will be separated from the alcohol, in proportion to the quantity of water added: is it not, therefore, probable, that they, (as I have before remarked) at the instant of their separation, preserve their attractive quantities of alcohol, through the medium of which they are enabled to unite; and thus finally effect the production of the oleaginous compound?

Theory of the formation of the compound.

I cannot, however, conclude this communication without entreating my reader to excuse the unfinished manner in which I have offered these few experimental facts and observations, from believing the relation of them may probably be the means of exciting some gentleman to examine the

6 subject

subject with more accuracy and precision, so as, I trust, to confirm in a more satisfactory manner the experiments, which I have ventured to bring forward, and achieve the principal object of the communication.

I am, SIR,

Your humble Servant,

S. B. H. F. R. GOWAR.

March 23, 1811.

III.

Comparative Analysis of Gum-Resins: By Mr. Henry Braconnot, *Professor of Natural History, &c.*

(Concluded from p. 237.)

Art. V. *Analysis of Olibanum.*

Action of heat on olibanum.

§ I. Olibanum is difficultly melted; and when kindled by the flame of a candle continues to burn of itself, leaving white ashes.

Distilled with water,

Twenty gram. [308·8 grs.] distilled with water yielded about 1 gram. [15·4] grs.] of volatile oil, of a pale lemon colour, and the smell of which is not unlike that of lemons.

and dry.

A similar quantity distilled with the naked fire gave a very large quantity of a brown empyreumatic oil, and an acid watery liquid in no great abundance. These products did not give any perceptible indications of ammonia on the addition of lime; but on bringing a little muriatic acid near the mixture some slight white fumes were visible, which appear to indicate the presence of volatile alkali. In the retort remained a compact coal, weighing 2·5 gram. [38·6 grs.] which left after incineration 55 cent. [8·5 grs.] of ashes, composed of 5 cent. [0·77 of a gr.] of potash partly saturated with sulphuric, muriatic, and carbonic acids; 6 cent. [0·93 of a gr.] of phosphate of lime; and 44 cent. [6·8 grs.] of carbonate of lime.

Heated with alcohol.

§ II. *A*. 25 gram. [386 grs.] of powdered olibanum were heated with a sufficient quantity of alcohol, and filtered. On the filter remained a pretty copious whitish sub- stance

stance, which, being well washed with alcohol and dried, weighed 9 gram. [139 grs.]

Residuum dissolved in water.

B. This matter insoluble in alcohol was dissolved in boiling water, with the exception of a soft grayish substance, of a gummy appearance, and weighing when dry 1·3 gram. [20 grs]. This burned with flame; and when treated with nitric acid, produced a greenish resiniform substance; which may lead us to suspect, that some resin had escaped the action of the alcohol, though I took great care to wash the residuum with boiling spirit of wine.

Gum.

C. The aqueous solution *(B)*, after being filtered, yielded by evaporation 7·5 gram. [115·8 grs.] of a gum, of which the following are the properties.

Its properties.

1. This gum, which was considered as extractive matter by the older chemists, though it has by no means the appearance of it, has a yellowish transparency, and a flat taste.

2. It dissolves easily in water, without leaving any residuum.

3. Exposed to the fire it burns with but little flame, and leaves white ashes, formed in great part of carbonate of lime.

4. Its aqueous solution does not redden infusion of litmus.

5. Oxalate of potash forms a precipitate in the solution.

6. Acetate of lead produces in it no very apparent change; but the nitrate, and more especially the subacetate of lead and the nitrate of mercury occasion in it very copious, thick, white precipitates, completely soluble in distiled vinegar.

7. The decoction of galls precipitates the solution of the gum of olibanum, but not that of the gum of myrrh.

8. Lime-water in excess does not affect its transparency, even after standing some time.

9. This gum is carbonized by sulphuric acid, but does not emit acetic vapours on heating the mixture. What then is it, that neutralizes the lime in this gum? I had a suspicion of benzoic acid, but this requires to be confirmed.

10. The gum of olibanum heated with nitric heat lets fall, particularly on cooling, a pretty large quantity of very white powder, granular as if crystalline. On continuing the

the evaporation to dryness, a residuum was obtained, which, when well washed and dried, furnished mucous acid, amounting to a third of the weight of the gum employed. The waters of elutriation contained oxalic and malic acid, but in small quantity.

Resin.

D. The alcoholic solution (*A*), evaporated to dryness, yielded 14 gram. [216·2 grs.] of resin; whence it follows, that there was a loss of 2 gram. [30·88 grs.], which I ascribe in great part to the volatile oil.

Its properties.

1. The resin of olibanum is of a reddish yellow colour; it easily cracks on cooling, becomes very brittle, has no perceptible taste, is electric when rubbed, and a good deal resembles common resin.

2. It softens in boiling water, but it requires a greater heat to melt it. Exposed to flame, it burns, and emits a smell that is not unpleasant.

3. This resin, heated with a solution of caustic potash, and evaporated to dryness, left a residuum not very soluble, which, after being well washed, was diffusible in boiling water, producing an emulsion. Common resin unites much more readily with alkalis *.

4. Sulphuric

* Resinous substances have never yet been examined with the minuteness and attention they deserve, as appears from the few facts collected respecting their chemical properties, and even of these several are inaccurate. Many even suppose, that alkalis have no action on resins; as a trial of which I made the following experiments:

Action of alkalis on common resin.

Into a cold and very weak solution of potash I put common resin powdered, by a little at a time. The result was a complete soapy solution, which became thick and ropy like the white of an egg. The weakest acids occasioned in it a copious white sediment. Alkalis and neutral salts do the same, in consequence of their greater affinity for water. A large quantity of common water too decomposes this soap, and the precipitate it occasions is but partly soluble in alcohol. The part that refuses to dissolve in it still contains a great deal of resin united with lime.

If muriate of lime be added to this resinous soap, the mixture thickens into a whitish mass resembling pap, consisting of resin and lime.

All metallic solutions completely decompose the soap of resin, and several of the precipitates thus formed may be employed in painting.

Fifty

4. Sulphuric acid dissolves the resin of olibanum without heat, forming a red solution, from which water throws down a white precipitate. If it be heated some time, water separates from it a black resin, soluble in nitric acid; which by evaporation converts it into a brown residuum of an astringent taste mixed with bitterness, and the aqueous solution of which precipitates gelatine, but not sulphate of iron.

5. If the resin be treated to dryness with eight parts of of nitric acid at 38°, it is almost wholly converted into the resiniform matter, the properties of which I have already made known. The waters of elutriation, which contained no oxalic acid, yielded by evaporation a residuum, which, when heated afresh with nitric acid, afforded none of the tanning matter discovered by Mr. Hatchett; but a bitter, soluble substance, partly precipitated on cooling from its solution in boiling water, soluble also in alcohol, and producing precipitates in several metallic solutions.

Converted by nitric acid into a kind of resinoamer.

This substance appeared to me to possess properties analogous to those of what I have termed resinoamer, but it differs from it in the stability and proportion of its elements.

ART. VI. *Analysis of Gum Ammoniacum.*

§ I. Gum ammoniacum, exposed to a heat not sufficient to decompose it, softens easily, and loses 6 per cent of its weight.

Action of heat on ammoniacum.

Twenty-five gram. [386 grs.] distilled with a naked fire yielded 12 gram. [185·3 grs.] of liquid, the greater part of

Fifty gram. [772 grs.] of resin, heated to saturation with solution of potash, dissolved entirely; and by evaporation were obtained 69 gram. [1065 grs.] of solid resinous soap, dry at a low temperature, of a brown colour, and which I found to be perfectly similar to Starkey's soap. Baumé and Legendre therefore had reason to presume, that only the thick and resinous part of oil of turpentine could really combine with potash; for this alkali acts on volatile oil only by promoting its conversion into resin to unite with it afterward.

Starkey's soap.

Volatile alkali, diluted with a great deal of water, also combines with resin very readily, and quickly reduces it to the saponaceous state. This compound diluted with water acquires after a time the thickness of starch.

which

which was a brown empyreumatic oil. This product emitted a smell of volatile alkali on the admixture of lime. In the retort was left a coal, weighing 7·5 gram. [115·8 grs.], which after incineration left 3·2 gram. [49·4 grs.] of ashes mixed with gravel. From these weak nitric acid extracted 1·5 dec. [2·3 grs.] of phosphate of lime, and 3 dec. [4·6 grs.] of carbonate of lime.

Ammoniacum boiled in alcohol.

§ II. (*A*) I boiled 25 gram. [386 grs.] of powdered ammoiacum with a sufficient quantity of alcohol at 36° [sp. gr. 0·837]. The solution filtered boiling let fall nothing on cooling. A white substance remained on the filter, which, after being washed with alcohol and dried, weighed 5·8 gram. [89·5 grs.].

Residuum dissolved in water.

B. This substance, being heated with water, dissolved in it in great part. The solution, being strained through fine linen, left a grayish substance, soft and as it were glutinous, insoluble both in water and spirit of wine, black after it was dried, and then weighing 1·1 gram. [17 grs.]. It burned more easily than a gum, and was reduced to gray ashes. Treated with nitric acid it gave a yellow colour, and afforded a little oxalic acid.

Gum.

C. The aqueous solution (*B*) yielded on evaporation 4·6 gram. [71 grs.] of gum.

Its properties.

1. This gum was transparent, of a reddish yellow colour, and a slightly bitter taste; somewhat brittle, and dried easily in comparison with other gums.

2. It burned without any apparent flame, leaving some white ashes, which dissolved in acids with effervescence.

3. Water dissolved it entirely; and the solution could barely be said to redden infusion of litmus.

4. It was precipitated entirely from its solutions by the subacetate of lead; but not at all by acetate, or the nitrate of lead or of mercury. The latter merely rendered the solution milky.

5. Lime-water in excess does not render the solution of this gum at all turbid, therefore it contains no phosphoric acid; yet its ashes yield a little phosphate. Hence I conceive phosphorus exists in this gum in a state of combination with the other elements that compose it.

6. Oxalate

6. Oxalate of ammonia throws down from the solution a precipitate of oxalate of lime, but I do not know by what acid the lime is neutralized in the gums*.

7. Decoction of galls detects nothing in it.

8. This gum, treated with the nitric acid of the shops, yielded nearly the same products as that of olibanum; that is, a large quantity of mucous acid, some oxalic acid, and a very little malic acid.

D. The alcoholic solution (*A*) being evaporated to dryness, yielded 17·5 gram. [270 grs.] of a resin, which exhibited the following characters. Resin.

1. It was of a reddish yellow, transparent, brittle as wax at a low temperature, having a wavy and shining fracture, receiving a slight impression from the nail, and readily growing soft in the mouth or between the fingers. It had no sensible taste: its smell was the same as that of ammoniacum itself. Friction did not render it electric. Its properties.

2. This resin, like wax, melts at 43° [129° F.]. At a higher temperature it swells up considerably, emits a peculiar smell, and leaves a light, spongy, bulky coal.

3. This resin easily combines with alkalis, even cold; and the results are saponaceous compounds of considerable bitterness †.

4. Sulphuric acid readily dissolves the resin of ammoniacum, particularly with a gentle heat. Water decomposes this solution. If it be heated more, the result is a hidroguretted coal; which, after being washed, dissolves in nitric acid, and yields by evaporation an astringent substance, which gives a brown precipitate with isinglass.

5. Nitric acid heated on the resin at first produced only some white vapours: but at length the red fumes appeared on a sudden with great vehemence. The product was a Treated with nitric acid

* Mr. Vauquelin, who has made known the existence of phosphate of lime in the ashes of gum arabic and gum tragacanth, supposes the lime in these gums is saturated in great part by the acetic acid.

† The soap or saponule of common resin has likewise a decidedly bitter taste, which seems to me to indicate in these resinous compounds efficacious properties, that deserve the attention of the observing physician.

yellow

yellow resiniform substance, which dissolved during the process. On evaporating to dryness I obtained a resinoamer substance of a very pure yellow, fusible at a gentle heat, uniting with alkalis, soluble in alcohol and in boiling water, and falling down in part from the latter on cooling; soluble in part too in a large quantity of cold water, and communicating to its solutions a very fine yellow colour, which adheres very tenaciously to the fingers, and is easily fixed on wool and silk, giving them a fine lustre unalterable by oximuriatic acid, and even by weak alkalis: consequently there is nothing that can be compared with it for permanence among the majority of dyeing drugs. I may add too, that this yellow appears to me to merit the preference before others in point of cheapness, considering the small quantity of it required.

formed a resinoamer.

Fine yellow dye.

This substance, mixed with gum, might yield a more pure and permanent yellow than gamboge.

Component parts of ammoniacum.

From this examination of gum ammoniacum it follows, that 100 parts are composed of

Gum - - - -	18·4
Resin - - - -	70
Glutiniform matter	4·4
Water - - - -	6
Loss - - - -	1·2
	100

IV.

On the Mechanism of Respiration in Fishes: by Mr. C. Duméril, *Professor at the Medical School, &c.**

The mechanism of respiration in fishes similar to that of some reptiles.

IN these papers I intend to explain some ideas respecting the act of respiration in fishes; to demonstrate, that the mechanism by which water is drawn into the mouths of these animals is precisely similar to that of the respiration of some reptiles; and that, in consequence of this mode of respiration, the seat and perception of the organ of smell

* Mag. Encyclop. Nov. 1807, p. 35. Read to the Institute August the 10th.

in

in fishes are altogether different from what they are in other vertebrate animals.

In the mammalia and birds the whole of the venous blood is under the necessity of coming to place itself in contact as it were with the air, that penetrates their lungs. These organs are enclosed, and capable of expanding, in the thorax; a cavity, the bony sides of which are capable of various motions, and particularly of those that tend to increase or diminish its diameters.

Respiration of beasts and birds.

In the mammalia the thorax appears particularly intended to protect the principal organs of the circulation; and to determine the motions necessary to respiration. In all other vetebrate animals, that have lungs, these parts are contained in a less determinate space, with the organs of digestion and other viscera.

Seat of the lungs.

The ribs, or bony hoops, that surround this cavity, are evidently levers, that mechanically produce the expansion or contraction of the space surrounding the lungs. The powers that move the bones in these two directions, and which are the muscles of the thorax and abdomen, and the diaphragm, consequently determine the act of inspiration, as well as that of expiration. It was necessary to recapitulate these general principles, to give a clearer notion of what follows.

Acting causes of respiration.

If it be true, as physiologists at present admit, that the ribs and muscles inserted in them are the chief mechanical agents of respiration; we must necessarily infer, that vertebrate animals destitute of ribs, or in which these bones cannot be employed to produce those motions, must have some other mode of breathing: and this in fact is the case.

Where ribs are wanting a different mechanism necessary;

The first example we have in certain reptiles, as frogs, lizards, &c., during the first stage of their existence. These animals, previous to their metamorphosis, breathe in the manner of fishes, of which they have commonly the form, structure, and habitudes. They take in water at the mouth, and immediately expel it through a single bronchial aperture, or several holes in the side of the neck. Thus tadpoles are similarly circumstanced with fishes of the genus sphagobronchiæ of Bloch, or unibronchaperturæ of Lacépède; while the young lizards, protei, sirens, and other urodelæ,

as in certain reptiles in their first stage, &c.

urodelæ, bear more resemblance to the sharks; particularly to that described by Schneider, and represented at fig. 31, in his edition of Bloch's System of Ichthyology; and which he has termed ciliaris, because a portion of the gills appears externally like a fringe.

The tadpole state sometimes long.

This period of the life of batrachian reptiles however is not so short, as has hitherto been supposed. Of this I have had an opportunity of satisfying myself, having kept tadpoles more than three years *. Yet when the lungs of these animals are completely formed, their gills lost, and they have only air to breath, there is hardly any thing altered on this account in their primitive organization, and they retain as we may say their old mode of respiration.

Reptiles with lungs, but no ribs, breathe in a peculiar way.

It is well known, that the batrachian reptiles have no ribs, or at most only the rudiments of these bones: the lungs therefore must be inflated by some mechanism different from that of other animals. This was conjectured by Herholdt and Rafn of Copenhagen, and almost immediately demonstrated by Cuvier and myself, though I have since corrected some of our former observations †. It is

Some reptiles slow in their changes.

* It has been said, that most of the batrachian species undergo their changes in one year: this is true with respect to the common frog, the edible frog, the green frog, the *salamandre abdominale*, and probably several others. It is known however, that the rana paradoxa is sometimes two years in the tadpole state; and I have observed, that the young marbled lizards retain their gills more than one year, having fed such in the spring. Besides, I have kept in my house for three years tadpoles of the *crapaud accoucheur*, which have constantly refused all kind of vegetable food, and which devoured one another, if they were not supplied with the flesh of animals.

Mode in which the nostrils are closed.

† There is neither valve nor valvulæ within or at the base of the nostrils. In all the anouri, that have the tongue free and its root attached to the cavity of the jaw, the two lobes apply themselves to the interior orifice of the nose, so as to prevent the issue of the air, or any other fluid contained in the mouth, at the time of deglutition. In the urodeli, which have no tongue, two tubercles, answering to the anterior bifurcation of the os hyoides, and covered by the inner membrane of the mouth, apply themselves to the two apertures of the nostrils, which are observed before the orbits of the eyes, on opening the mouth of any of the salamanders.

now

now known, that the respiration of the batrachian reptiles is effected by means of the muscles of the throat, and the elastic plate of the hyoides, which perform the office of ribs and diaphragm. And for inspiration thus to take place, the mouth must previously be closed; so that if an adult salamander or frog were placed in water, with the mouth kept open by force, or even left so in the moist and open air, it would soon die of suffocation. In all these animals the air is expelled suddenly from the lungs by the action of the abdominal muscles. It issues from the mouth in very large bubbles, and by a sort of vomiting, which explains the force and continuance of their croaking, even under water, as is observed in a great many species.

Tortoises.

All the chelonian reptiles are circumstanced precisely as the batrachian. Not that the tortoises for instance are destitute of ribs, or sternum, as no animals have these parts more solid and distinct. But all these ribs are united to the vertebræ and to each other, to form that bony shell, which covers their muscles, limbs, and viscera, in the back; as the parts of the sternum spread abroad cover them on the belly.

Mistake of Townson respecting their breathing.

I believe I first demonstrated in my public lectures, on living tortoises, the mistake of Townson in his examination of the mechanism of respiration in a tortoise, the under shell of which he had removed, and in which the mechanical action of the lungs was still exerted. This gentleman ascribes the faculty of drawing the lungs outward to two muscles, which can only compress them; while the motion of the throat, and the complete cessation of the act of inspiring when the mouth is open, would have given him a better insight into the true mechanism of this function, and the analogy that exists in this respect between tortoises and frogs. Indeed this is an observation, to which we are naturally led; and Mr. Cuvier, who has verified it, adopts the same opinion in his Lectures on Comparative Anatomy, vol. iv, p. 368.

Still these respire air, but by a different mechanism.

Here then we find the act of respiration performed in certain reptiles differently from what it is in animals that seem to precede them immediately in the scale of beings. Yet, though the mechanism is different, the effect is nearly

the same, since in both cases air is equally introduced into the lungs, there to produce and undergo a certain action. The real difference is only in the mode of inspiration, and the connexion of its continuance with the motion that empties the lungs. In fact, the air, inspired by small quantities in succession, is always introduced at the nostrils, and issues from the mouth at a single discharge of longer or shorter duration. During the stay of this large quantity of air inspired in the lungs, which seem to have been greatly expanded for the purpose, it divests itself of the oxigen it contains, so that the moment of expiration is to a certain degree arbitrary.

Difference of structure between these and fishes.

Most of the circumstances that have been mentioned will be exhibited at large in the following examination of the respiratory apparatus of fishes. All vertebrate animals, that breathe air alone, have only two apertures in the lower part of the mouth. One, intended for the passage of food, is the œsophagus: the other, admitting the passage of the air into and out of the lungs, is the glottis. Fishes on the contrary, without exception, have the throat perforated with four, six, or seven large apertures on each side, beside the passage leading to the stomach. These apertures supply the place of the glottis; and are commonly furnished on their edges with fringed appendages, which fit into each other, but are capable of being separated to a greater or less distance, to strain the water that passes through them. Each of these apertures directs the liquid into a space bordered by two bronchial laminæ, which are covered by a membrane, in the substance of which are commonly solid radii capable of motion, and a thin plate called the operculum, which can be moved so as to form a chink on each side. This aperture alone furnishes an issue to the water, which by staying on the bronchiæ has served for the respiration of the fish.

Manner in which fishes respire.

When a fish respires, it begins by opening the mouth, to suck in the liquid in which it is immersed. The water immediately comes in to fill the vacuum produced by the separation and protraction of the labial bones. The bottom of the throat is then raised, and the bronchial apertures frequently closed. But presently the lips approach each other;

other; all the hinder part of the head is enlarged by the raising of the opercula; the bony arches, that support the bronchial laminæ, are acted upon by the hyoides, which is depressed as in the act of swallowing; these arches separate from each other, and the water immediately enters into the spaces between them; but, at the moment these opercula are raised and separated from the body, the hyoides and all the bronchial laminæ connected with it rise against the arch of the palate, and drive before them all the water contained in the cavity of the mouth, which passes entirely round the bronchiæ, and is commonly discharged by the exterior apertures.

Thus we see the respiration of water by fishes is a real deglutition, but incomplete, because the mouth is perforated at bottom, and suffers the liquid, which necessarily enters with all its food, to escape.

This explains how fishes can swallow air in pretty large quantity *, because the specific gravity of gaseous fluids carries them to the highest part of the mouth, which is seldom perforated: this being the reverse of what happens in animals that respire air, which cannot naturally swallow it, because this fluid escapes by the posterior opening of the nostrils; while they can all swallow water without any difficulty. Fishes can swallow air.

The cetaceæ, which from their nature are obliged to reside in water, are circumstanced precisely the same as fishes. Every time these animals open their mouths to seize their prey, they are under the necessity of expelling the fluid introduced into them; and accordingly they are furnished with a peculiar organ, analogous in its effects to that of fishes. By the help of this organ they expel the water through their nostrils. Cetaceæ.

All the muscles, that serve in fish to expel the water from the cavity of the mouth into that of the gills, are the same as those that produce deglutition in other species of animals: only the apparatus is much more complex, as will Muscles of respiration in fishes.

* This commonly happens in the balistes and tetrodontes, which come to the surface of the water to suck in air, and thus puff themselves up considerably.

 readily

readily appear on comparing the structure of the os hyoides and bronchiostegous arches, which represent its horns, or elongations.

The motion that produces the expulsion of the water is altogether independent of that of the mouth and throat; though it is its common consequence, and occurs alternately, and is nearly isochronous with it. The powers that occasion the expulsion of the water are equally situate about the head; and it is chiefly this apparatus of bones and numerous muscles, that renders the heads of fishes so bulky, for they are frequently more than a third of the whole length of the animal. The bronchiostegous arches, included in the substance of the membrane, take the place of the true ribs: and in those species that want these bones, muscles in the shape of pouches fulfil the same office; acting on the water nearly in the same manner as the auricles of the heart on the blood, which they propel into the ventricles; or as the muscular bags found in the spiracles of whales.

This theory confirmed by apparent deviations.

The theory I have laid down seems to be confirmed even by the anomalies, that occur in some fishes, in which respiration appears to be effected in a different way, and which consequently exhibit some slight modifications in its organs.

Two peculiarities.

The common phenomena of the respiration of fishes have already been described: but there are some, as the plagiostomes and cyclostomes among the cartilaginous fishes, which do not constantly take in water at the mouth; and others, in which respiration may be termed arbitrary in its movements. These two peculiarities, which I shall examine somewhat at large, seem to show great analogy with the reptiles already noticed.

1st, in fishes, that frequently lie at the bottom of the water.

The rays, sawfish, and many of the squali, when they make no effort to sustain themselves in the water, naturally sink to the bottom, and lie on the sand: and as their mouths are in the under surface of the body, if they opened them like other fishes to draw in water, they would necessarily suck up gravel and other foreign matters, by which their gills might be wounded. On this account they seem to be able to breathe in the manner of frogs and adult salamanders,

ders, nature having furnished them with two apertures at the top of the head communicating directly with the throat. These are real nostrils, placed behind the eyes, but formed to admit the water into the mouth, whenever the throat dilates. The return of the liquid in the same direction is prevented by valves; and the animal, performing the action of deglutition without opening the mouth or œsophagus, forces the water into the cavities of the gills.

2d, in fishes that adhere to others, or to solid bodies, by their mouths.

The mode of respiration too must be different, but for another reason, in the lampreys and gastrobronchi, the only species destitute of true jaws; and which have been classed perhaps erroneously among the vertebrate animals, from which they differ considerably, as I intend to show in a paper that I shall soon have the honour of laying before the class.

Many of these species are parasitical: they fasten themselves by the mouth to other fishes, or fix themselves on stones and other solid bodies by the help of this part and a kind of suction. The water therefore must necessarily enter by some other orifice, that of the mouth being closed. In fact we perceive on their head, or in their lips, a patulous orifice opening to a canal, which directs the water into the throat as often as a vacuum is produced in it. The motion of deglutition that follows forces this water to penetrate into peculiar cavities, in which the bronchiæ float; without this fluid being able to issue out by the same orifice, though Bloch ascribes this faculty to them, and, as well as Rondeletius, has figured this sort of fountain in several of the plates of his superb work.

These similar to reptiles of the frog kind.

All these fishes then are circumstanced precisely the same as the batrachian reptiles, since they inspire water at the nostrils, and by deglutition force the fluid into the pulmonary cavity, from which it is afterward discharged by a different orifice.

Peculiarities where respiration is in some degree arbitrary.

We find too some peculiarities of conformation in other species, the respiration of which is as it were arbitrary, at least as to the duration of the three periods, or movements, that compose its mechanism, namely inspiration, deglutition, and expiration. Besides, these circumstances produce,

duce, as we shall see presently, a result nearly similar to that I have mentioned in the tortoise.

Fishes that live habitually in mud.

There are fishes that live habitually in mud and sand; some to lay snares for the species on which they feed, as the lophii, siluri, murenophides, &c.; others to shelter themselves from the attacks of more voracious fishes, as the ammodytes, callionymi, batrathoides, cotti, &c. In general in all these inhabitants of the mud we find the cavity of the bronchiæ considerably expanded, in order to receive a larger quantity of water, which is kept as it were in rerve: we perceive also, that the radii of the bronchiostegous membrane are considerably elongated, and the external aperture of the bronchiæ is very small in proportion to the extent of the cavity. All these circumstances are very naturally explained, when we know these species, like frogs and tortoises, require several successive aspirations, to fill with water that kind of bag, in which the bronchiæ are immersed, and which can be emptied at once, when the fish has no longer need of remaining in ambuscade, or comes into pure water to inspire a fresh portion.

An additional organ in such as can live some time out of water.

We find an addition to the common organization in those species of fishes, which in certain peculiar circumstances can leave the water, and remain on land, or in the mud of half-dried ponds, for a larger or shorter time. I am not speaking here of the false bronchiæ, which Mr. Broussonet has very well described under the name of pseudobronchia, and which has since been found in several fishes; but of a peculiar organ, a vascular expansion analogous to lungs, which lies loose in a cavity appropriated to it, but communicating with that of the common bronchiæ, from which it receives water. Consequently this is an accessory organ, similar in its uses to the air-bags observed in the chameleon, and in all birds. It is true we are yet acquainted with few instances of this remarkable arrangement; but I shall point out five, that are very striking.

Instances of this.

I shall first mention the *cephalopholis scansor* of Tranquebar, [*perca scandens*], described by Daldorf in the 3rd volume of the Linnean Transactions. This fish, which is in the Museum of Natural History at Paris, has the faculty of climbing trees by means of the spines, with which the different

different parts of the opercula are furnished. The *osphromenes goramy*, the organization of which has been described and figured from Commerson by de Lacépède, and which appears to possess the same organ, as far as may be judged from the structure and analogy. The *macropteronotus sharmuth*, the supplementary bronchiæ of which have been described by prof. Geoffroy in the 3rd volume of the Bulletin des Sciences. The *tetrodon Honkenii*, figured by Bloch under the name of *hérisson tigré*, which has exhibited the same organs to Mr. Schneider. And lastly the *hydrargyrus swampine* Lac., which Mr. Bosc found in Carolina, and of which he has given us some specimens for the Museum. In this fish I have just found these organs.

Connexion between the mechanism of respiration and deglutition not extraordinary.

From the preceding facts it appears to follow, that the mechanical act of respiration in fishes is similar to what takes place in several reptiles, and that the motion of respiration is necessarily combined with that of deglutition. This connexion between the organs however need not surprise us, when we observe under other circumstances, and for another object, this same function of respiration necessarily combined in the reverse order with the act of deglutition. Thus the adult elephant cannot drink, if he do not make a strong expiration into his trunk at the moment of deglutition. Other functions might afford us many analogous examples; but these considerations, though highly important, would be misplaced here, and carry us too far from our subject.

It occurs in the elephant.

V.

Farther Experiments on the alkaline Strength of the Product of the Oxigenation of Potassium, with Observations on the Nature of the alkaline Metalloids: by J. Murray, *Lecturer on Chemistry, Edinburgh.*

To Mr. NICHOLSON,

Edinburgh, April the 1st, 1811.

SIR,

Object to determine the comparative strength of

IN the communication inserted in the last Number of your Journal, I gave an account of some experiments, made with the view of ascertaining the comparative alkaline

strength

common potash and that from potassium.

strength of the potash formed by the combustion of potassium, and of fused potash prepared by the usual process. The facts, that fused potash contains a considerable portion of water, and that in its decomposition by galvanism there is not that evolution of hidrogen which might be expected from the decomposition of this water, led me to suppose, that hidrogen enters into the composition of potassium, and that the potassium in burning must form an alkali containing water; and the most direct mode of determining this appeared to be the ascertaining, whether the product of the combustion of potassium is the same or not in alkaline strength with common ignited potash. As this is an investigation of considerable delicacy, and the precise determination of it difficult, I have since been occupied in repeating and diversifying my former experiments.

Chief sources of errour.

The principal sources of errour, which attend the investigation, appear to arise from the difficulty of converting potassium into potash by combustion, without its acting on the substance with which it is in contact; and from the various degrees of oxidation of which it is susceptible, and which are not easily determined.

Potassium acts on platina when burned on it.

In burning potassium on platina the latter metal is acted on, and a portion of its oxide remains mixed or combined with the potash formed. I have noticed the insoluble powder which remains, when the product of the combustion is dissolved in water, and which is probably principally oxide of platina. This substance having been collected and carefully weighed, and the weight deducted from that of the product which had been dissolved, it did not appear probable, that it could give rise to any important errour. Still it was desirable, if possible, to avoid its operation; and I endeavoured therefore to find some other substance on which the potassium would not act, but without success.

So it does on china,

and on silver;

In burning it on chinese porcelain, the surface is eroded; and on afterward dissolving the alkali in water a light gelatinouslike precipitate, probably silex, falls down. In burning it on silver there is also a mutual action between the silver and the potassium indicated by a small quantity of precipitate, when the product of the combustion is dissolved in water. It appeared to me however, that the action

action both on the porcelain and the silver is considerably less than that on the platina, and I employed them therefore in the following experiments.

but less than on platina.

Experiment with potassium burned on silver.

Seven grains of potassium were burnt on a silver basin. During the combustion a portion was dissipated in a dense white smoke, and the product did not exceed 7 grains; it was of a grayish colour, and fusible, remaining soft and nearly liquid, at a low red heat. Five grains of it were weighed while warm, and were dissolved in water. The solution was nearly transparent, depositing only a few flocculi. It was neutralized by an acid, composed of one part of nitric acid, by *weight*, and twenty parts of water, and of which 5 grains of ignited potash neutralized 139 grains *. The quantity required for neutralization, after separating the insoluble matter, and allowing as accurately as possible for its weight, was equivalent to 144 grains to 5 grains of the product free from this matter, and similar results, with differences of not more than 2 or 3 grains, were obtained on several repetitions of the experiment.

Potassium burned on china.

Seven grains of potassium were burnt on a cup of chinese porcelain. The product was lighter in colour than in the preceding experiments, being of a yellowish gray, nearly white. Five grains of it were weighed while warm, and dissolved in water. A flocculent gelatinouslike precipitate subsided, which, as it was found to be dissolved in a preceding experiment by a very weak acid, was removed before neutralization, dried, and weighed. The solution required for neutralization, of the diluted nitric acid, a quantity equal to 141 grains to 5 grains of the product free from this insoluble matter.

The product but little superior to common fused potash.

These experiments then, in common with those I have formerly stated, appear to establish the conclusion, that the potash produced by the combustion of potassium is very little superior in the power of neutralizing an acid, to the common fused potash. It is with much caution that I have admitted this conclusion, as the opposite result has been supposed to be established, on the authority of some eminent

* The acid which I used in my former experiments was composed of one part of nitric acid by *measure* with 20 parts of water.

eminent chemists. But, after taking every precaution to guard against any source of fallacy, I have been able to obtain no other result. If the product of the combustion of the potassium were the real alkali, 5 grains of it ought to have neutralized 162 of the above acid, since 5 grains of ignited potash, containing at least 14 per cent of alkali, neutralized 139 grains.

But potassium may be oxided in different degrees.

The only circumstance which I can discover, as leaving any room for doubt, is that of the different degrees of oxidation, of which potassium is susceptible. There is reason to believe, that a compound may be obtained at a lower degree of oxidation than potash; and from the more recent experiments of Gay-Lussac and Thenard, the general result of which has been stated by Mr. Davy, it farther appears, that another compound, in which the potassium is more highly oxidated than it is in potash, may be formed. This last substance, they have stated, contains three times more oxigen than potash does; this excess of oxigen is expelled by exposure to a red heat, or when the product is dissolved in water, it is disengaged. If this substance had been the one operated on in the above experiments, it might give rise in part to the apparently equal alkaline strength of the potash formed by the combustion of potassium, and ignited potash; as a given weight of it would contain less real alkali than when this excess of oxigen was not present. But the circumstances connected with the experiment lead me to believe, that no important errour from this cause could have operated. The product of the combustion of the potassium was always kept for some time at a red heat, before it was operated on, by which, according to Gay-Lussac and Thenard, this excess of oxigen is expelled; and except in one experiment no effervescence was perceived on dissolving it in water.

This might have affected the result,

though it did not appear to be the case.

The experiments favour the opinion, that potassium is a compound of a metal with hidrogen.

If these sources of errour have not operated, and the experiments be correct, they appear to me to support the opinion I have advanced with regard to the nature of potassium. Were it the real base of the alkali, forming it when combined with oxigen, the compound thus formed must be superior in alkaline strength to common potash, which contains so considerable a proportion of combined water.

Excluding

Excluding the hypothesis of Gay-Lussac and Thenard, there appears to remain only one other view to be adopted—that potassium is the metallic base of the alkali, combined with hidrogen, the real alkali being a metallic oxide, which in common potash is combined with water. In its decomposition the oxigen both of the oxide and of the water is separated, while the metallic base and the hidrogen, or part of the hidrogen, combining, form potassium; and in the combustion of this product, its base and its hidrogen unite with the oxigen consumed, forming an alkali combined with water, and therefore not superior in alkaline strength, at least not to a great extent, to the potash in which water is combined.

The experiment made in a different mode

Wishing to avoid every source of fallacy, I thought of a different mode of conducting the experiment, and this I have to state has afforded a result somewhat different. Mr. Davy had inferred from the combustion of potassium in oxigen gas, that 86·1 of it combine with 13·9 of oxigen to form 100 of potash. From its oxigenation by water, he had stated the proportions at 84 of base and 16 of oxigen. The former appeared to be the most direct mode, and I therefore preferred the estimate founded on it. Supposing then 86 of potassium to unite with 14 of oxigen to form 100 of potash, I took 4·3 of potassium, which according to this estimate are equivalent to 5 of real potash, and exposed it (the vessel being covered with a slip of muslin) to a humid atmosphere, until it was converted into a solution of potash. This I found required for neutralization, taking the mean of several experiments, of which the results varied very little, of the diluted acid employed in the above experiments 154 grains.

appeared to give a different result.

Here the power of neutralization in the alkali, formed by the oxigenation of potassium, supposing it to consist of 86 of base, and 14 of oxigen, is considerably superior to the power of neutralization of an equal weight of ignited potash, the one being 139, the other 154. It appears to have been by this mode of experiment, that chemists have determined the comparative alkaline strength of oxigenated potassium, and fused potash; at least I have seen no accounts of any attempts to do this by experiments on the real product

product of the combustion of potassium. Hence the agreement in the conclusion, that the alkaline strength of the former is superior to that of the alkali in its real state.

Calculation of the quantity of acid, that should have been neutralized.

To perceive more clearly to what conclusion this result, supposing it to be correctly established, leads, it may be calculated what quantity of acid the quantity of real alkali from 4·3 of potassium ought to neutralize. Five grains of ignited potash, containing 14 per cent of water, neutralize 139 grains; five grains of real potash, free from water, ought, according to this, to neutralize 162 grains; which is the quantity of course which the 4·3 of potassium ought to have neutralized, on the supposition that in combining with oxigen it forms the real alkali.

Inferences.

Now although the alkali produced from the oxigenation of potassium is thus inferior to real potash in neutralizing power, a result which so far supports the speculation I have advanced, yet it appears to be superior considerably to the ignited potash; and this may be supposed therefore to invalidate the first series of experiments, in which the alkali formed by the combustion of potassium was found little superior to ignited potash. To admit of this latter conclusion, however, it must be supposed, that the proportions in which potassium and oxigen combine to form pure potash are accurately estimated; and this can scarcely be considered as determined with perfect certainty. No experiment is more exposed to sources of errour, both from the different degrees of oxidation of which potassium appears to be susceptible, and the action which it exerts during its combustion on the substance with which it is in contact. These become too, of much greater imporportance in the experiment, and even the accuracy of the estimate, independent of them, becomes more doubtful, from the smallness of the quantities which have hitherto been operated on. If from any of these circumstances the proportion of oxigen has been underrated, even to a small extent, the difference between the result of the above experiment, and of that in which the alkaline strength of the product of the combustion of potassium was directly ascertained, will be accounted for.

Objections to these.

Another

Another circumstance is to be attended to, of much importance with regard to the conclusion, so far as it relates to the general theory. The estimate of 14 per cent of water in ignited potash is the lowest that has been made. Berthollet himself, on whose authority it rests, has considered it probable, that there is a larger quantity; the abstraction of the whole, even by its decomposition, being prevented by the powerful affinity exerted to the last portions. By d'Arcet the proportion has been stated at 27 in 100 parts. Mr. Davy has inferred from an experiment, in which potassium was converted into muriate of potash in muriatic acid gas, that fused potash must contain nearly 23 per cent of water; and more lately he has remarked, that, if the proportion were to be judged of from the loss of weight in combining common potash with boracic acid, it would appear to be from 19 to 20 per cent. If we take the lowest of these numbers, 20, then the five grains of ignited potash, containing 20 per cent, neutralizing 139 grains, 5 grains of real potash ought to neutralize 174, and this is the quantity that ought to have been neutralized by 4·3 of potassium, supposing it to form the real alkali by oxigenation.

The calculation farther affected by the water in potash not being precisely determined.

The result, therefore, from this mode of experiment cannot be considered as invalidating the result from the more direct mode of trying the alkaline strength of the actual product of the combustion of potassium, and even by itself is not incompatible with the conclusion, that a portion of hidrogen exists in potassium, but rather serves to establish it.

The result, therefore reconcilable with the hypothesis.

The circumstance, that there is no sensible evolution of hidrogen in the decomposition of common potash by galvanism, still appears to me to go far towards establishing the opinion, that hidrogen enters into the composition of potassium. Mr. Davy, in reply to Mr. Dalton, who had stated this in support of the opinion of Gay-Lussac and Thenard, has lately remarked, that, in using a weak galvanic power, such as that which he employed in his first experiments, and by which small quantities only of the metals are procured, there is no sensible effervescence; but that, when from 500 to 1000 plates are used, there is a violent effervescence, and a production of hidrogen, and sometimes of potassuretted hirogen,

Argument from no evolution of hidrogen

opposed by its evolution under certain circumstances.

But this accounted for by the decomposition of the water in the potash.

hidrogen, connected with the formation of the metal. This, I think, admits of a different conclusion from that which is intended to be drawn from it. Water is certainly more easy of decomposition by galvanism than potash is; when the galvanic power therefore is sufficiently strong to decompose the latter, it must be able to decompose the former; and of course, when common potash, containing water, is decomposed by a galvanic battery, this water can scarcely be supposed to escape decomposition. And the hidrogen of such a portion of water cannot be supposed to pass off unobserved, for although the hidrogen in water is to the oxygen only as 15 to 85 by weight, yet it is as 2 to 1 nearly by volume: and hence in the decomposition of water by galvanism, the disengagement of elastic fluid at the negative side is much more abundant than at the positive. It is farther to be considered, that, from the small quantity of oxygen which enters into the composition of potash, and the large quantity of it which exists in the composition of water, the quantity of it belonging to the water, which common fused potash contains, supposing this to be only 15 per cent, is equal to the qnantity which is contained in the real alkali. If both, therefore, are decomposed in the galvanic action, half the quantity at least of the oxygen disengaged at the positive wire must be from the water, a quantity of hidrogen double the quantity of this, that is equal in volume to the whole oxigen evolved at the positive side, ought to appear at the negative side; if an effervescence, therefore, is perceived at the former from the disengagement of oxigen, an equal effervessence ought to be conspicuous at the latter; and, since it is not, we can scarcely avoid the conclusion, that the hidrogen, or at least much of it, enters into the composition of the globules of metallic matter, which appear at the negative side. With regard to the fact, that when the potash is under the action of a very powerful battery, there is a violent effervescence, and production of hidrogen, and potassuretted hidrogen, I suppose this must arise from the intense heat excited at the point of decomposition, by which an elastic combination of potassium and hidrogen is established. If there is a certain degree of galvanic energy, at which hidrogen, pure, or not holding any

Farther arguments for the existence of hidrogen in the metalloid produced.

any potassium in solution, is expelled, it would deserve to be made the subject of experiment whether the metal obtained is precisely the same with the metal procured when there is no sensible disengagement of hidrogen; though still it is possible, that in all cases a portion of the hidrogen of the water in fused potash may be disengaged, which becomes only more evident when a large quantity of the alkalis is decomposed by a powerful galvanic battery. It can scarcely however be supposed, that the whole of the hidrogen is disengaged, else it must be apparent, if the above observations be just, even in the experiment where the weakest power is employed, for the volume of elastic fluid to be disengaged at the one side, if the water is decomposed (and of this decomposition scarce a doubt can be entertained) is as large as that disengaged at the other.

When potassium is procured by means of iron, the hidrogen may be expelled from that portion which combines with the iron.

In the method of procuring potassium by the action of iron on potash, at the temperature of ignition, a disengagement of hidrogen gas takes place during the process; a fact which may be supposed to prove at least, that the whole hidrogen of the water contained in fused potash does not enter into the composition of the potassium which that potash yields. This may, perhaps, be admitted; it is possible, as I have already remarked, that a portion only of the hidrogen exists in the potassium. Yet in drawing this conclusion one circumstance is to be attended to. The greater part of the base of the potash is obtained, not insulated, but in combination with iron. The hidrogen of this latter portion may be expelled, the iron combining with the real metallic matter, and this may afford the greater part of the hidrogen that is disengaged.

Compounds of potassium and hidrogen formed.

Potassium has an affinity to hidrogen. It combines with that gas so as to form an elastic compound; and, according to the experiments of Gay-Lussac and Thenard, it also combines at a high temperature with a large quantity of hidrogen, and forms a solid substance of a light gray colour, from which hidrogen is disengaged by the action of water. Mr. Davy was unable to obtain this result; but these chemists, aware of the objection he had made to their experiment, have more lately repeated it, and have affirmed, that, when potassium is heated in hidrogen gas, a quantity of the gas

gas is absorbed, and a solid compound of potassium and hidrogen formed. These facts, as proving the existence of an affinity between potassium and hidrogen, are favourable to the supposition, that it may consist of a base combined with a portion of hidrogen; especially as potassium is never formed, where hidrogen is not present. Sodium is scarcely visibly soluble in hidrogen; it appears therefore to have a less strong affinity to it, and may be supposed to retain a smaller quantity in combination with it. And from this may arise, in conformity to the view I have given in my former paper, the greater specific gravity of sodium, and the inferior attraction of soda to water.

Sodium has less affinity for hidrogen.

The lightness of the metalloids is probably owing, I have stated, to the combination of real metals, which form their bases, with hidrogen; the lightest of them, potassium, probably containing the largest portion. It is a curious fact in conformity to this view, that the two lightest metals, tellurium and arsenic, are those which have the most peculiar relation to hidrogen, and evidently exert an affinity to it. Ritter observed the singular fact, that, when tellurium is employed as the negative wire in decomposing water by galvanism, hidrogen gas is not evolved, though oxigen is given out at the positive side; at the negative a powder falls down, which he found reason to conclude is a compound of tellurium and hidrogen *. The observation is an important one, as proving a combination to be established by galvanism between a metal and hidrogen, though in this case the quantity of hidrogen combined appears to be so large, as to cause the metallic qualities to disappear; as happens too in the combination of potassium with a large proportion of hidrogen in the experiment of Gay-Lussac and Thenard above quoted †. I have found, that, in decomposing water by galvanism, if arsenic be the metal immersed in the water at the negative side, while platina is at the positive side, less

The two lightest of the common metals combine with hidrogen.

Arsenic combines with hidrogen by galvanic action.

* See Journal, vol. xxiv, p. 318.

† These facts with regard to the loss of metallic properties when hidrogen in considerable proportion is combined with a metal are unfavourable to the hypothesis, that it is a metallic body in the aerial form. The properties of its oxide, water, are equally adverse to this exposition.

Hidrogen not an aeriform metal.

hidrogen

hidrogen is evolved, than when platina forms the negative wire; and apparently less even than the quantity of gas discharged at the positive side. A portion of the hidrogen therefore must be retained in combination with the arsenic; and if arsenic acid slightly moistened be submitted to galvanic action in a platina cup connected with the positive side, on touching it with a platina wire from the negative side, dense white vapour arises, which is probably not from mere volatilization, but from the action of hidrogen on the arsenic. It deserves to be remarked too, that arsenic is of any of the metals the one most soluble in water in its oxidated state.

Its oxide most soluble in water.

All the facts I have stated in this paper appear to concur in giving probability to the view I have ventured to propose of the nature of potassium and the other metalloids—that they are metals combined with hidrogen.

I have the honour to be,

Yours, &c.

J. MURRAY.

VI.

On the Affinity of Muriatic Acid for different Bodies, considered with Regard to MR. DAVY'S *Ideas of its Nature. In a Letter from a Correspondent.*

To MR. NICHOLSON.

SIR,

THE more I reflect upon Mr. Davy's new Theory of the nature of oximuriatic acid, the more I am convinced, that it is incomplete, and inadequate to explain the mode, as well as the nature of its combination with different bases. You were good enough to place in your Journal for last month some observations of mine relative to its inapplicability to some phenomena, which occur in the production of the hyperoximuriate of potash. Permit me now to call your attention to some facts relative to the affinity of muriatic acid for different bodies, which appear to me to be totally subversive of this ingenious hypothesis.

Remarks on the attraction of muriatic acid for different bodies.

Decomposition of muriate of soda by sulphuric acid.

Muriate of soda, according to Mr. Davy, is a compound of oximuriatic acid and metallic sodium. When sulphuric acid is added to it, it is decomposed by complex affinity. The water, which is contained in this acid, furnishes hidrogen to the oximuriatic acid, which thus becomes muriatic; and oxigen to the sodium, which is then saturated by the sulphuric. This explanation appears at first sight perhaps satisfactory. But let us examine it nearer; water we know, is composed of oxigen and hidrogen. The *proportions* of each are *fixed* and *determined.* Sodium we likewise know combines with oxigen, as upon the new hypothesis does oximuriatic acid with hidrogen. But the proportions in which these unite are also *fixed* and *limited.*

Now is it not *primâ facie*, at least, a very extraordinary circumstance, that the quantities of oxigen and hidrogen, which exist in water, should exactly correspond with the proportions in which sodium combines with the one and oximuriatic acid with the other? And we must suppose this the case. The component parts of water, thus separated, must either enter wholly into the new compounds formed, or they will make their appearance each or either in its own peculiar form. In the instance before us neither of them appears in this form, therefore we must look for them in the new combinations. Now is it not a very uncommon coincidence, that there is no surplus in any of these ingredients? That the entire quantity of oximuriatic acid liberated from the muriate of soda agrees so exactly with the hidrogen which is set free from the water, that the whole quantity of both form muriatic acid; while *at the same time* the entire quantity of sodium likewise liberated tallies as exactly with the remaining oxigen?

Decomposition of nitrate of silver by muriatic acid.

An inverse instance of the same improbability occurs, when muriatic acid is added to a solution of nitrate of silver. Here a precipitate is formed, which, according to Mr. Davy, must be oximuriatic acid and silver. Hidrogen is disengaged from the muriatic acid, and oxigen from the oxide of silver, which existed in the nitrate, in quantity sufficient, neither superabundant, to form water.

But we need not argue on improbability,

But we are not thus left to argue upon *improbabilities:* we have it in our power to show whether such reciprocal change

change of parts does take place or not. We know the relative quantities of the ingredients in the compounds, which we originally submit to reciprocal action, and these quantities we must account for. But if, from established data, and collateral evidence, we know that the constituent parts of the original compounds cannot wholly combine in the actual proportions in which they are known to exist in the new forms, and if we have no means of accounting for any such surplus or deficiency, the hypothesis must fall to the ground. we may form calculations.

Let us thus endeavour, by the aid of long established and well authenticated analysis, to explain the synthetic operations, which take place in the instance above alluded to, viz. the decomposition of muriate of soda by sulphuric acid. Example in the decomposition of muriate of soda by sulphuric acid,

Water, which fulfills so important a function in these new arrangements, is composed of 85 per cent of oxigen, and 15 per cent of hidrogen, (in round numbers.) Muriate of soda consists of 59 parts basis, and 41 acid. Oximuriatic acid unites, according to Mr. Davy *, with its own bulk of hidrogen, to form muriatic acid. The relative weights of these must then be nearly 97 † parts of the former, and 3 of the latter. Sodium, upon the same authority, combines with 22 per cent of oxigen to form soda.

Now let us suppose, that 97 parts of oximuriatic acid are disengaged from their union with the sodium, by the action of the sulphuric acid ‡. These 97 parts will instantly seize upon three parts of the hidrogen of the water, and form muriatic acid. Thus there will remain 135 parts of sodium, and 17 parts of oxigen. These ought to combine to form soda, which the sulphuric acid would then saturate. But

* See Journal, vol. xxvii, p. 325.

† Gay-Lussac states oximuriatic acid to be heavier than common air, in the proportion of 247 to 100, which, taking the weight of 100 in that of the latter to be 31 grains, gives 76·5 for the weight of the former.

‡ The manner in which the sulphuric acid acts is by no means satisfactorily accounted for. No affinity can possibly subsist between it and *metallic* sodium; by what complicated agency then is the oximuriatic acid first disengaged?

upon the scale above laid down, here is a deficiency of no less than 21 parts of oxigen for the complete oxidation of this quantity of sodium.

If we suppose, that upon the action of the sulphuric acid the sodium first becomes saturated with oxigen, the difficulty will not be diminished. For in this case there would be an immense surplus of hidrogen, above what was necessary for the formation of muriatic acid.

and in that of nitrate of silver by muriatic acid.

Thus likewise, in the solution of silver in nitric acid, before noticed, the oxide contains 92 parts of metal, and 8 of oxigen. Twenty parts of muriatic acid are required to saturate these 92 parts, which only gives 0·6 of a part of hidrogen to combine with the 8 parts of oxigen. But by the foregoing proportions, 1·4 of hidrogen are required for the complete conversion of this quantity into water.

The proportions above laid down I have taken from the best authorities; and I believe that the calculations founded upon them are correct. But even supposing it possible to adjust either of the above examples to the exact relative quantities necessary to support Mr. Davy's hypothesis, yet, instances of the like discrepancy are so numerous and so various in their particulars, that to attempt to reconcile them is a hopeless task.

Argument from the combination of muriatic acid with metallic oxides.

But there is another objection, which is more simple and more self-evident, than that above stated. In the case where the oxides of metals give up their oxigen to combine with the hidrogen of muriatic acid to form water, as the quantity of hidrogen in muriatic acid must be definite and limited, the quantity of this acid required to saturate the different oxides ought to be in *direct* and *exact* proportion to the quantity of oxigen which they contain. But this is not the case. The oxide of potassium contains 13 per cent of oxigen, and combines with 36 per cent of muriatic acid. The oxide of sodium contains 22 per cent oxigen, and ought, therefore, in this proportion to unite with 60 per cent at least of acid. But 41 per cent will be found to be the utmost limits of its union.

It is useless to multiply examples. One such objection, if founded in fact, would be sufficient at once to overturn the longest established, and, in other respects, the com-

pletest

pletest theory. I will, therefore, trespass no farther on the limited space of your valuable Journal. By giving a place in it to the above observations, you will render an important service to the cause of chemistry; as they must either draw from the ingenuity of Mr. Davy a reconciliation of the differences, and thus confirm, by undeniable and irresistible evidence, the justness of his ideas; or they must inevitably confirm the insufficiency of his ingenious hypothesis. For I am certain, that no one can for an instant doubt the justness of the principle upon which they are founded, viz. that in the case where two compound bodies are submitted to mutual decomposition, the elements of these bodies must either be totally absorbed, and (if I may be allowed the term) neutralized by a fresh combination; or they must appear in their own peculiar form, and with their own distinguishing characters.

I remain, Sir,

Your most obedient servant, and constant reader,

13th of April, 1811. F. D.

VII.

Description of an economical Process for Evaporation, invented by the late Mr. Joseph Montgolfier*: by Messrs.* Desormes *and* Clément *.

Spontaneous evaporation applied to economical purposes.

CONSIDERING the great effect of spontaneous evaporation, that takes place from the simple contact of air with humid substances, or with water itself, Mr. Montgolfier imagined, that the same means might be employed for drying without the assistance of fire a great number of matters, which are liable to be injured by its application. The instances of natural salt-pits, and graduation houses, occurred to him, in which evaporation is conducted very economically: but the former are adapted only to hot climates, and the latter depend too much on the state of the atmosphere, to be applicable to the principal object he had in view, the desiccation of alimentary substances; an operation of which the object was to preserve them a long time without

* Abridged from the Annales de Chimie, vol. lxxvi, p. 34.

without alteration, and without the addition of sugar, salt, or other matters, which are expensive, and alter their natural flavour.

Concentration of must for making wine in northern countries;

He had formed, in particular, the project of concentrating the must of grapes previous to its fermentation, in order to convey it at small expense to northern countries, there to make it into wine by the addition of a suitable quantity of water, and then fermenting it. The idea was grand, and perhaps at some future period will be realized: but in the mean time the fabrication of grape-sugar is an important object for such a process.

This process requiring but a very moderate temperature, which may be that of the atmosphere alone, will render the sirup of grapes exempt from the empyreumatic flavour commonly found in it, though it does not exist in the fruit: and probably occasion its use to become more extensive, thus contributing to the accomplishment of the benevolent views of government.

and of the juices of fruits. Improvement in sugar.

It is equally applicable to the evaporation of all the juices of fruits, and would be particularly advantageous to that of the sugar cane, as it would render the raw sugars much whiter, and more pure than those that usually come to us so loaded with an uncrystallizable melasses, which gives them a bad taste.

Other applications of it.

The economy of the process too allows its application to every process of evaporation, that does not require a high temperature. It might be employed in the salt-works in the east of France, in saltpetre manufactories, &c.

Delicious conserves in large quantity.

Montgolfier's first trials were made in 1794. He prepared conserves of several fruits, and among others of apples and grapes. The former, which was in large quantity, for he made above three thousand pounds weight, had so fine a flavour, that the fruit itself appeared disagreeable in comparison to it. He repeated his experiments at Paris, in 1797, with results equally satisfactory, except that the products were not so good as those afforded by the fruits of Dauphiny; indeed there seemed to be more difference between the conserves than between the fruits of the two countries.

The publication of the hydraulic ram *, and its improvements, turned his attention from the present subject, which, though

* See Journal, vol. xiv, p. 98.

though important, was less interesting, and he thought no more on the subject. The results, however, are too valuable, particularly at the present moment, to be kept longer secret.

Principle of the process.

As the air has the property of evaporating water at all temperatures, we have only to bring a large quantity into contact with the wet matter we would dry, or the liquid we would evaporate, to obtain a great effect in a little time.

If we would obtain this effect in a small space, which is the object of almost all manufacturers, we must cause a great deal of air to pass through this space in a little time by producing a rapid current. A very simple machine, not expensive, and well adapted to this purpose, is known by the name of a centrifugal ventilator*.

Thus in evaporation, without artificial heat, the caloric necessary for the production of vapour will be that of the atmospheric air: but it will be the free caloric, and not that of combination, which combustibles alone can disengage. To produce the union of the free caloric of the atmosphere with water, simple contact alone is required; and this contact may be effected by power. Power, therefore, is the only expense required, to obtain this combination of water with caloric. It will be shown presently, that this is much cheaper than fuel: besides, the former is always at our disposal, while the latter becomes every day more expensive, in consequence of the consumption of wood and of charcoal. We have therefore a real advantage, general and particular, in the process of evaporating without artificial heat.

Advantages.

Heated air may be employed,

Sometimes the atmospheric air is so damp, that it would dissolve but little water, and its being set in motion would profit but little. Such cases, however, are very rare; and we see every day, even during rain, that the wind is drying. But to hasten evaporation, and regulate the process, the air may be heated before it enters the evaporating machine: it might be passed through a fire of charcoal, if the subject under evaporation would be injured by smoke; or of any

* It is described by Desaguliers in the Philosophical Transactions for 1735; but he does not appear to have assigned the best possible dimensions to the machine.

combustible

combustible in other cases*. We should thus obtain the same advantage as by employing the atmospheric air at its proper temperature. With the air heated by the fire cold air might be mixed, so as to make the temperature of the mixture 30°, 40°, or 50° [86°, 104°, or 122° F.] at most. Beyond this we are not certain, that the juices of fruits would not be altered: at least Montgolfier stopped at this point in his experiments.

with a saving of fuel.

Though in this case fuel is employed to transmit to the water the latent heat of the air, there is still a saving of expense. Here the caloric evolved by combustion is well mixed with the air, which carries off almost the whole of it, and easily parts with it to the water, from the vast surface this presents in the apparatus to be described. Besides, if the air contained a little free caloric before the combustion, it would still present it to the water as in the evaporation without artificial heat; and if it escape at a lower temperature, than it had when it entered, we may be sure we have employed the fuel to good purpose. In the ordinary mode of evaporation we are far from enjoying these advantages: the caloric is obliged to traverse substances of considerable thickness and little surface, to combine with the water; and the air escapes rapidly from its contact with the surfaces, and carries with it a considerable quantity of heat.

Calculations of the effect.

We have here no new idea, but the result of well known truths; the advantages of which have been overlooked. To form a proper judgment of it, we should know with Montgolfier, that, in the common state of the air in autumn, a cubic foot of air, by its contact with water, will evaporate from one to four grains, or at a medium $2\frac{1}{2}$ grs. We will suppose it only 2 grains, the proportion adopted by Montgolfier from his experiments in Dauphiny. To put the air in motion, like any other body, requires more power for a greater velocity; and this power is proportional to the square of the velocity. That is, if a man by a day's labour, as is the fact, can give a velocity of $7\frac{1}{2}$ feet [8 feet Eng.] per second to 8100000 cubic feet [9800000 Eng.],

* The air to be heated should not be passed through the naked fire, but through a tube or tubes heated by the fire. C.

it

it would require as much power to give twice the velocity to one fourth the quantity of air.

This velocity, or that of 16 feet per second to 2450000 cubic feet of air, is well adapted to the present case, as it does not require too great a power, and the dimensions of the apparatus will not be inconvenient. We will suppose a man to work at this rate for an hour, then to rest as long, and after six hours of actual labour to have accomplished his day's work. Having thus passed over the evaporating matter 2450000 cubic feet of air, he will have produced an evaporation of 4900000 grs, or rather more than 100 wine gallons of water; and, as the pay of a day-labourer at Paris is only 1 f. 50 cent. [15d.], such a quantity might be evaporated at this expense. To evaporate a similar quantity by fire would take about two bushels of coals, which at Paris would cost from 2s. 6d. to 3s. The labour of a horse would be rather more expensive than that of a man, and would require a larger apparatus to be applied to the best advantage: but where a larger machine is desirable, a greater moving power is requisite.

Having explained the principles of the process, we shall proceed to describe the apparatus; observing, that not merely the dimensions, but the arrangement of several of its parts may be considerably varied, without detriment to the effect.

Pl. X, fig. 1, is a perpendicular section of the machine; and fig. 2 is a plan of it in the line A′ B′. Apparatus described.

A B is a handle $15\frac{1}{2}$ inches long, making a circuit of 49 inches, which a man turns round in a second.

B C is an axis, having at its extremity C a toothed wheel, taking in a cogwheel D, which has twice as many cogs as the wheel has teeth, so that it makes but one turn while the wheel makes two.

The cogwheel D is fixed on an axis D E, which rests by a pivot on the support E, and is kept vertical at F by a brass collar, well fitted to it, but admitting it to turn with ease.

The axis D E carries six or eight wings 4 feet 10 inches long. The ribs of these are of iron, and they are covered with oilcloth, or varnished cloth: for they should be both strong and light.

This

Apparatus described.

This mill turns between two large circular planes, which extend a little beyond the ends of the wings. These planes form the top and bottom of a kind of drum, the circumference of which is open, except two hoops, one at top, the other at bottom, about 6 inches broad; and consequently leaving between them an opening of 371 inches by 7·86, or 2916 square inches; for I suppose the distance between the top and bottom of the drum to be 19½ inches.

The wings of the ventilator should be as broad as possible, so that they can move without friction.

The top and bottom of the drum are connected of course by pieces of wood at their circumference, which will diminish the opening. This may accordingly be estimated at 2400 square inches only: but this is of little importance as it is sufficient if it be equal to the inhaling orifice, which we shall estimate at 1000 square inches.

The bottom of the drum has a circular hole, G H, at least 36 inches in diameter. The free surface of this should be 1000 square inches.

To this is fitted a cylindrical tube, I K, of the same diameter, and descending perpendicularly about 40 inches. It is supported so as not to rest on the lower part of the apparatus.

L M is a lid for the square box N O P Q. It is perforated with a hole corresponding to the tube I K, and from this hole rises the end of a cylinder similar to that above, which fits exactly. Paper is to be pasted over this joint, that no air may escape.

The box N O P Q is a rectangular prism, 8 feet 2 inches square, by 5 feet 9 inches high, in which are placed twigs of birch, without bark, and very clean, in regular beds, crossing each other alternately, and leaving more open space at bottom than at top. On this heap is dispersed the liquid to be evaporated, which enters by small holes in the lid, so as to be distributed as equally as possible amongst the twigs.

Between the twigs and the lid a space should be left free in the middle at least a foot high, not to check the passage of the air. The twigs also should not be too close, but placed so that no horizontal section should contain less than 3000 square inches nearly.

The

The bottom of the box is a wooden grating, the bars of which are very open; and about three feet beneath it is a vessel to receive the concentrated fluid. There it will receive the first impression of the open air, and be still more concentrated. Apparatus described.

In the case we have noticed, when the air is not sufficiently drying, it may be heated by passing it through a furnace, the chimney of which widens to at least 1000 square inches, and takes in fresh air by sufficient openings for the purpose. This pipe should be divided into four others of 250 square inches each, communicating with the openings at the bottom of the four sides of the box.

After the description here given, it is obvious, that the ventilator, turned by the man, throws the air from between its wings to the circumference, and draws in fresh air at its centre, which air traverses the whole heap of twigs, and consequently the fluid. If this be not sufficiently concentrated in one descent, it may be pumped up again, and suffered to fall as often as is necessary.

Montgolfier attempted to do without the twigs, by forming small jets of fluid in the box, the height of which he increased: but this mode is not economical with regard to expense of power. They might be dispensed with, however, by adopting the following contrivance. In the lower vessel, that receives the fluid, place a wooden roller, turning on its axis. At the top of the box, which in this case should be long and narrow instead of square, place a similar roller, with a winch affixed to it without. Over these two rollers pass an endless band of cloth, and this, on turning the winch, will raise the fluid, and expose it to the air on an extensive surface.

SCIENTIFIC NEWS.

Wernerian Natural History Society.

AT the meeting of this society, on the 16th of February, Professor Jameson read a minerealogical description of the romantic and interesting valley of Pentland, near Edinburgh. Valley of Pentland, near Edinburgh.

burgh. After describing the magnitude and direction of the valley, and the shape and height of the mountains surrounding it, the Professor stated as a probable conjecture, that it was formed by a lake, which had emptied itself by a lateral valley that traverses its south side. He next enumerated and described the various rocks of which it is composed, and gave a full detail of their geognostic relations. Four sets of rocks were described as occurring in this valley; transition rocks, as sandstone and conglomerate rocks,—rocks of trap-tuff,—wacke,—amygdaloid, &c.; and rocks of the clay-stone series, as clay-stone, porphyroid, compact-feldspar, &c.

Other communications.

At the meeting on the 9th of March, the Secretary read communications from Dr. Thomas Thomson, containing an analysis of iron-ore from Greenland; from Dr. Edmondston, Shetland, on the larus glaucus; and from Dr. Barclay, on the structure of the cells of bees and wasps.

Culture of Jerusalem artichokes.

The Free Society of Arts of the Department of the Sarthe, has offered prizes for those who shall have cultivated with most success Jerusalem artichokes, and mangel wurzel, not less than an acre of each. It is observed that the Jerusalem artichoke is particularly advantageous as winter food for Merino sheep, which are exceedingly fond of them. They are best given in the proportion of two pounds with one of dry fodder. A field, that would produce but 1500 rations of lucerne, will yield 5000 rations of Jerusalem artichokes. The stalks are good fuel, or when green may be given to cattle. Cows, hogs, and horses, eat the root as well as sheep. The plant is liable to no injury from drought in summer, or frost in winter; and no insect or disease has been found to check its vegetation.

Prize for flax-spinning machine.

In May last the Emperor Napoleon published a decree, dated from Bois-le-Duc, in which he offers a prize of one million of francs [£41666] to the inventor of the best machine for spinning flax. The prize is offered without any restriction in regard to time, or to the country of the inventor.

Grape sugar.

Mr. Proust having extracted from the sirup of grapes a concrete sugar; and Mr. Fouques having found means of whitening it, and giving it the colour and consistency, though

though not the sparkling appearance [*le brillant*] of sugar from the cane; a reward of 100000 francs, [£4166] has been bestowed on the former, and 40000 francs, [£1666] on the latter. The sweetness of this sugar, however, is much inferior to that from the cane, as in this respect one pound of the latter is equivalent to at least 2¼ of grape sugar.

Congreve's rockets.

At a meeting of the Italian Academy, in January, 1810, Mr. Hess, of Zurich, read an account of a method of preventing the effects of Congreve's rockets.

Tin mines discovered in France.

In 1795, wolfram, which commonly accompanies veins of tin, was discovered in France, near St. Leonard, in the department of the Upper Vienne, and some search was in consequence made for tin, but without success. Lately, however, farther search has been made by Mr. de Cressac, mine engineer, and he has found tin in the state of oxide, and characterised by its peculiar crystallization. Mr. Descotils has analysed it, and obtained from it very pure tin. The vein was found to be more rich, in proportion as it was traced to a greater depth, and it is expected may be worked with advantage.

New land discovered.

Mr. Hedenstroem, in the Russian service, discovered in the summer of 1809 a land in the Frozen Ocean, to which he gave the name of New Siberia. The part he explored he has called the coast of St. Nicholas. His observations will be found interesting to the Natural Historian, as well as to the Geographer.

Fresco painting transferred to canvas.

Palmaroli has succeeded in removing the Interment of Christ, by Daniel de Volterra, from the wall of the church of the Trinity on the Mount at Rome, on which it was painted in fresco; and transferring it to canvas, to be conveyed to Paris.

Substitute for tobacco.

The dried leaves of the cherry-tree are now employed by the Norwegians as a substitute for tobacco.

Preparations of phosphorus.

Mr. Curaudau procures phosphorus by mixing one hundred parts of calcined bones, thirty of potash, twenty of sulphur, and fifteen of vegetable charcoal, and distilling the mixture in a stone retort by a strong heat. The phosphorus that comes over commonly contains a little sulphur, but it is not the less fit for the fabrication of

of phosphoric matches, which require a mixture of this substance.

Improvement of the tone of wind instruments.

A musician of Avallon, in France, professes to have discovered a method of giving to all kinds of wind instruments made of wood that improved tone, which they generally acquire only after having been used for a considerable time, and which is afterward liable to be impaired. It imparts to a new instrument all the perfection of one that has been ten years in use, and renders it permanent.

Soles of shoes fastened by screws.

A Frenchman, who professes to make screws superior in quality to the best of English manufacture, recommends them strongly for fastening the soles of boots and shoes. He says they occasion a saving of three fourths, from the great durability of the shoes thus made; and that they do not make a noise like nails, as their heads imbed themselves in the leather.

Treatise of mathematics.

A complete course of pure Mathematics, for the use of the pupils of the Normal and Polytechnic Schools, has been published at Paris, by Professor L. B. Francœur, in two volumes. Its method is much commended.

Meteorological Journal.

Professor Vassalli Eandi commenced, in 1809, a meteorological Journal, under the title of Annals of the Observatory of the Academy of Turin, with statistical notes relative to Agriculture and Physic. It appears to embrace much information from its plan, which is as follows: 1. The days of the month, the moon's age, daily declination, phases, apogee, and perigee; height of the barometer, and of two thermometers, one facing the south, the other the north, at sun-rise, noon, and sun-set. 2. Observations on the anemoscope, anemometer, hygrometer, hydrometer, and atmidometer, at the same hours. 3. State of the atmosphere at the same periods. 4. Summary of the meteorological observations for each month, with the extraordinary phenomena, if any occur. 5. Comparison of these observations with meteorological sayings, both general and relating to the state of the moon. 6. Natural epochs, namely, the appearance of birds of passage and insects, and rural observations relative to the operations of agriculture, the flowering of plants, the harvesting of different crops, the devastations of insects, and the congress and births of domestic animals,

animals. 7. A Comparison of these with agricultural sayings, and the physical grounds of these sayings. 8. Marriages and births. 9. Diseases and deaths. 10. Diseases and deaths of domestic animals. 11. Price currents of different articles, with the reasons of variations in their price. To these it is the editor's intention to add, 1. Observations of the electrometer, magnetic needle, diaphanometer, cyanometer, photometer, and eudiometer. 2. A comparison of the meteorological aphorisms of Toaldo, Lamarck, and others, with the observations made. 3. Proceedings of the most skilful and active farmers for every month in mountainous districts, and in plains. 4. Bee journal. 5. Remarks on the daily bill of mortality, and the age and condition of the deceased, particularly with regard to sudden deaths, and the character of epidemic or dangerous diseases. 6. Observations similar to those made at Turin, carefully registered in the other departments of the 27th military division. 7. A comparison of these with similar observations in the departments of Montblanc and the Maritime Alps, in the 28th military division. 8. Various other observations, calculated to enlarge the boundaries of science. 9. Astronomical observations, as soon as the observatory is furnished with the requisite instruments.

Entomology of Switzerland.

Mr. de Clairville has published a second volume of his Entomologie Helvétique, the first of which appeared in 1798.

Memoirs of the Royal Academy of Munich.

The Memoirs of the Royal Academy of Sciences of Munich for 1808 contain several papers on botany, and some on comparative anatomy. Mr. Petzl has found the Bolognian spar in some marle pits near Amberg. He has also described a calcareous carbonate, lying under a thin coat of vegetable mould near Erding, in Bavaria, and there called *alm*, or *alben*, which destroys the fertility of the soil for some years, if it be turned up by too deep ploughing. According to him it is a calcareous tufa in a state of efflorescence, and should rank between chalk and bergmehl. We should suspect, from the experiments of Mr. Tennant on our limestones, that it contains magnesia. Some observations on direction and inclination of the strata in the primitive mountains of Norway and Sweden, by Mr. J. F. L. Haussmann, tend to confirm the theory of Mr.

Mr. von Humboldt. Dr. C. F. Bucholz, having attempted to analyse a sulphate of zinc containing copper in the mode proposed by Mr. Vauquelin, has proved its insufficiency. He immersed a slip of zinc in a solution of the sulphate, and found, that the zinc, instead of precipitating metallic copper, acquired a blackish coating, which, when separated, dried with blotting paper, and rubbed with an agate burnisher, had the colour and brightness of brass.

Defence of fortified Places.

A treatise on the Defense of fortified Places, composed by order of the Emperor, for the Instruction of the Pupils of the Corps of Engineers, by Mr. Carnot, is a work of considerable importance in the military art. In an appendix he strongly recommends to the besieged, to fire howitzers loaded with grapeshot, or musket balls, at an elevation of 45°, when the enemy have made their approaches within a certain distance. They may thus fire from behind the ramparts, without being exposed; and their shot will do more execution, not being stopped by the enemy's works. This is now made a part of the French artillery exercise.

New Encyclopedia.

In the beginning of last year was announced at Paris a prospectus of an "Instructive Collection of all the theoretical and practical Truths hitherto discovered in Natural History, Physics, Chemistry, Medicine, Surgery, Agriculture, Arts and Trades, and Domestic Economy, subjected to fresh Examinations, verified by new Observations, and confirmed by new Experiments." The work is to be illustrated by upward of a hundred and fifty thousand plates, many of them coloured, representing accurately all the mineral substances that can be delineated, all known plants and animals, all the machines, inventions, instruments, and tools hitherto contrived, in France or other countries, with portraits of persons who have distinguished themselves in the arts or sciences. It is to be the joint work of a great number of literary men and artists of the various parts of Europe; and is to be composed in four separate languages, French, English, Italian, and German, by new methods, the object of which is to render its execution equally prompt and perfect, and to enable persons of the smallest fortunes to procure this useful collection at a trifling expense.

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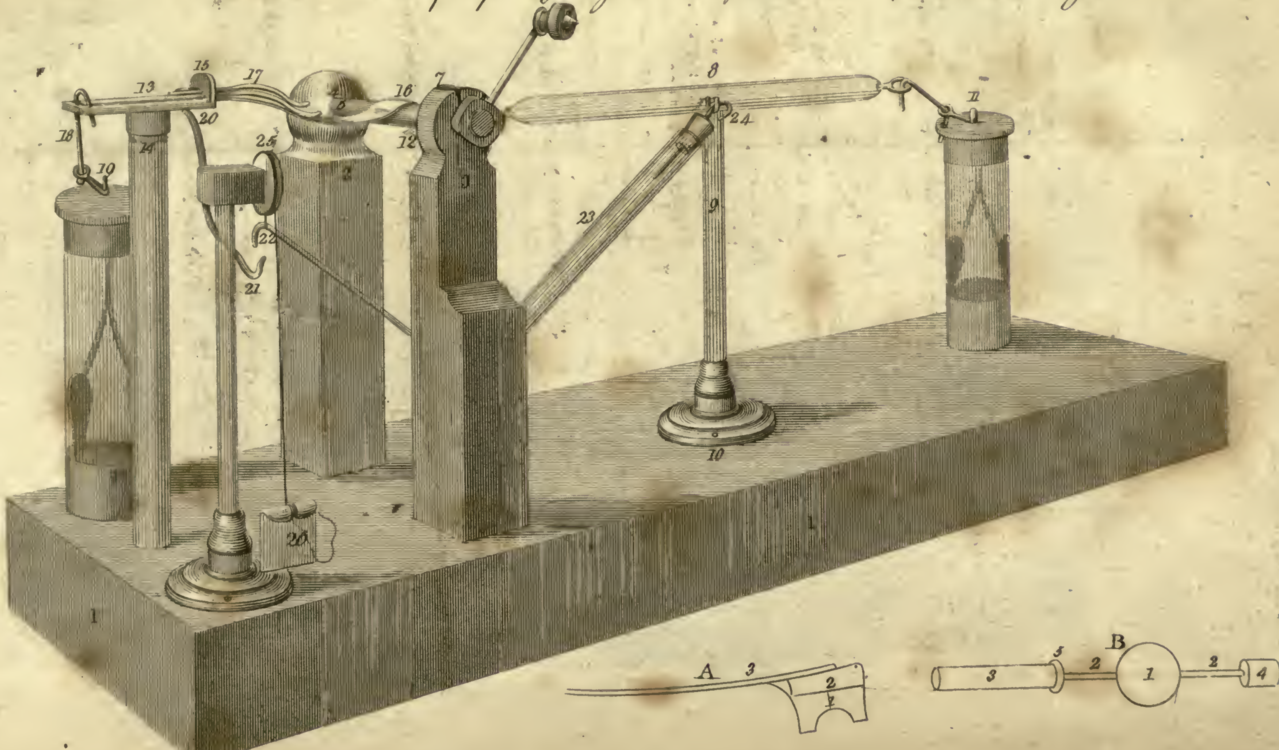

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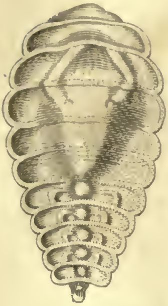

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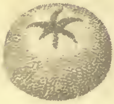

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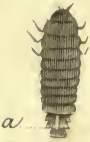

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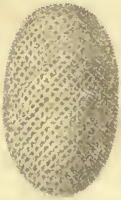

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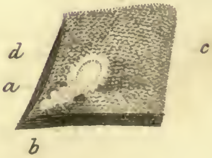

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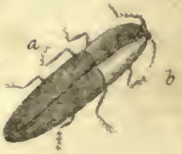

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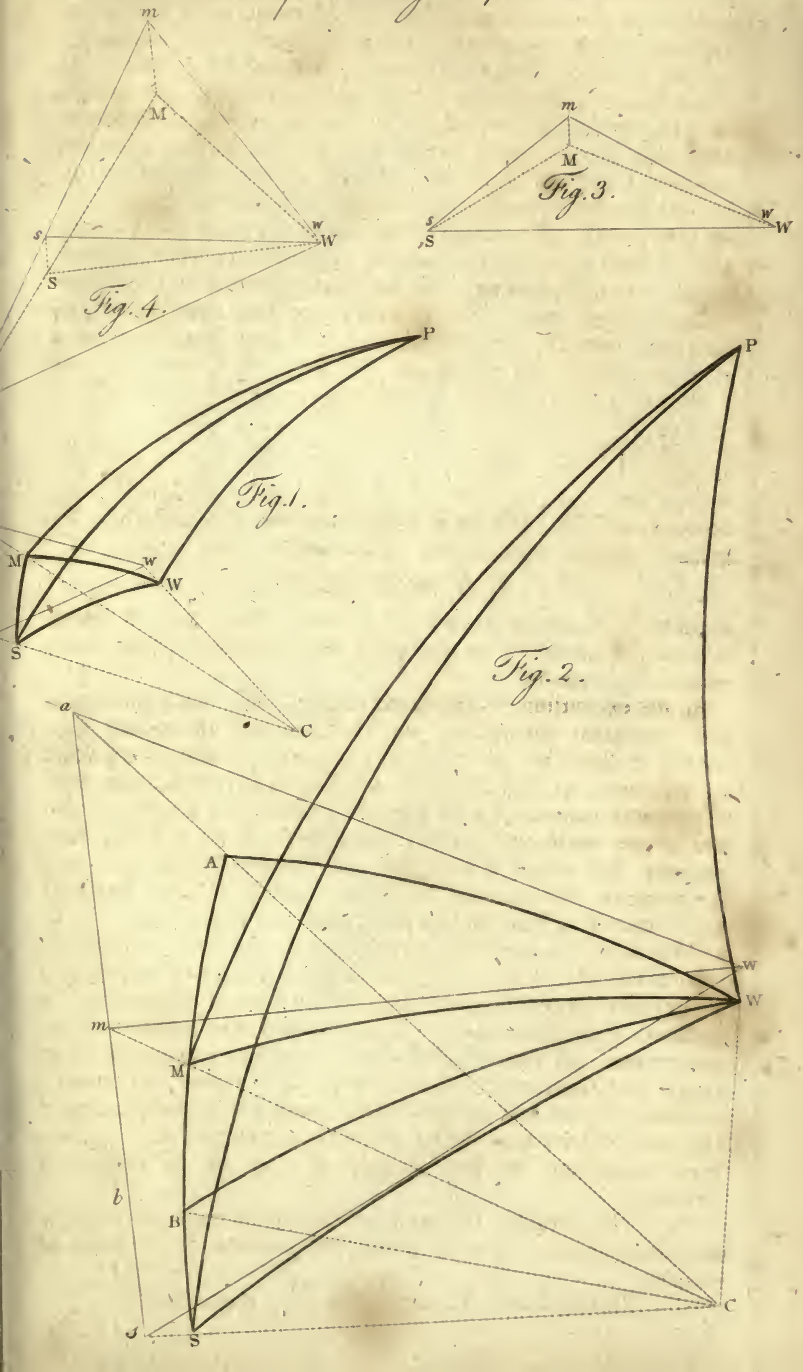

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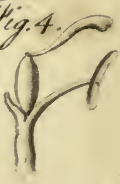

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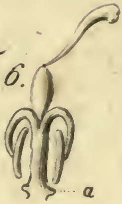

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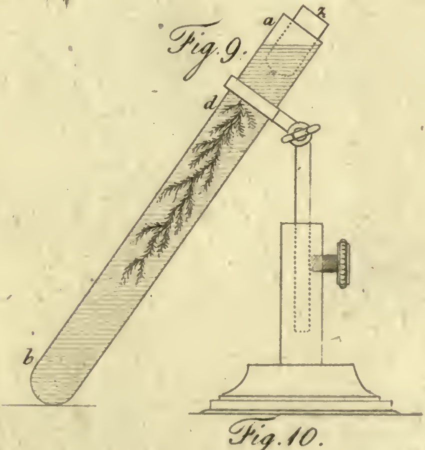

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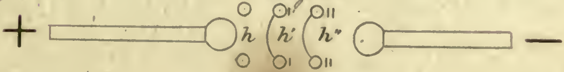

Motion of Rockets.

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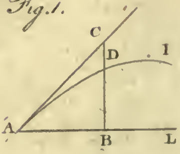

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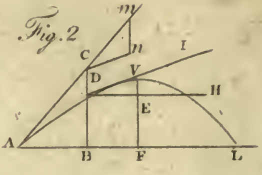
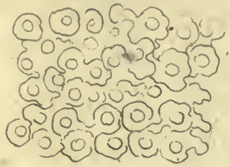


Structure of the Petals of Flowers

Fig. 3



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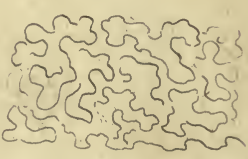

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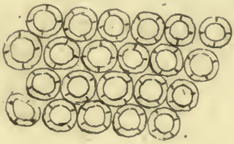

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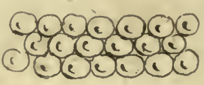

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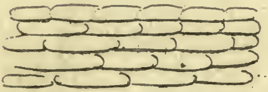

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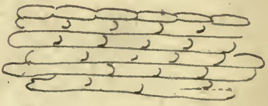

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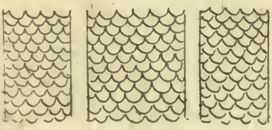

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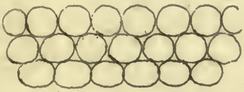

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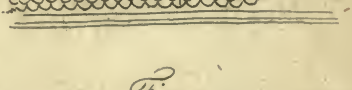

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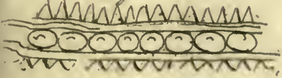

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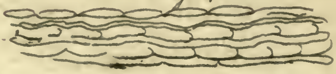

Corollas of Flowers.

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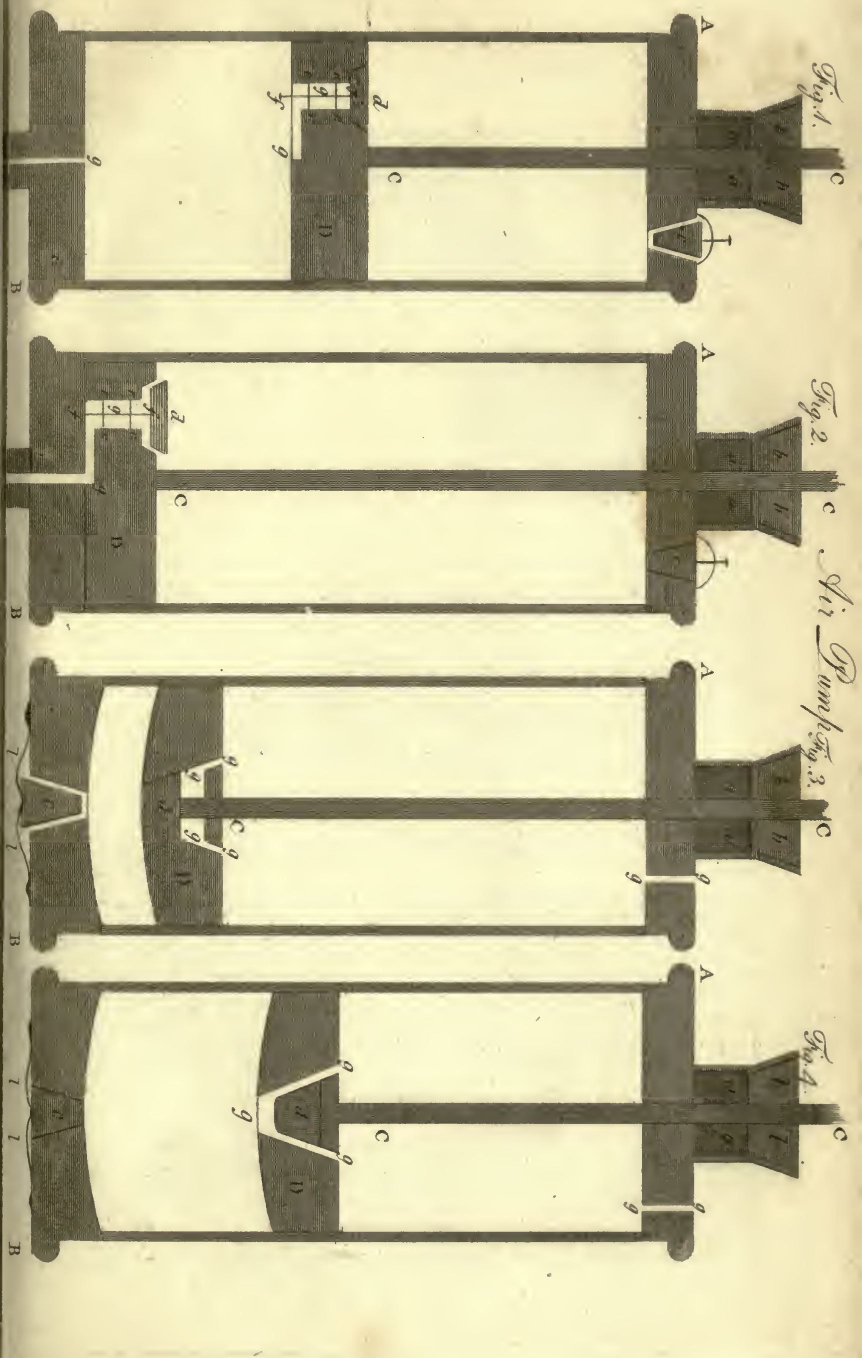

Air Pump.

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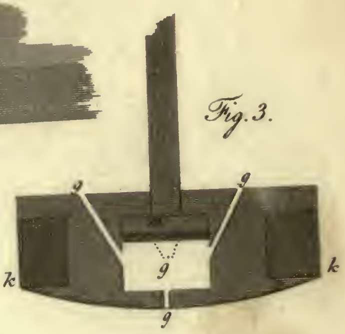

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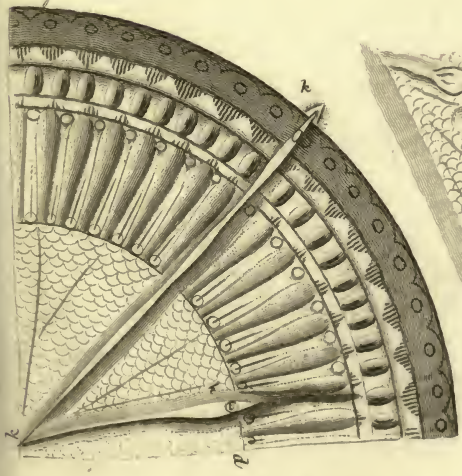

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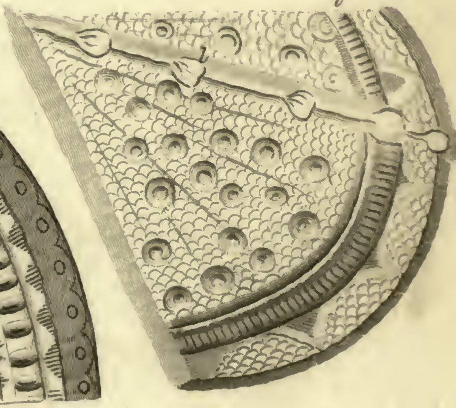

Structure of Plants

Machine for evaporating Fluids without Heat

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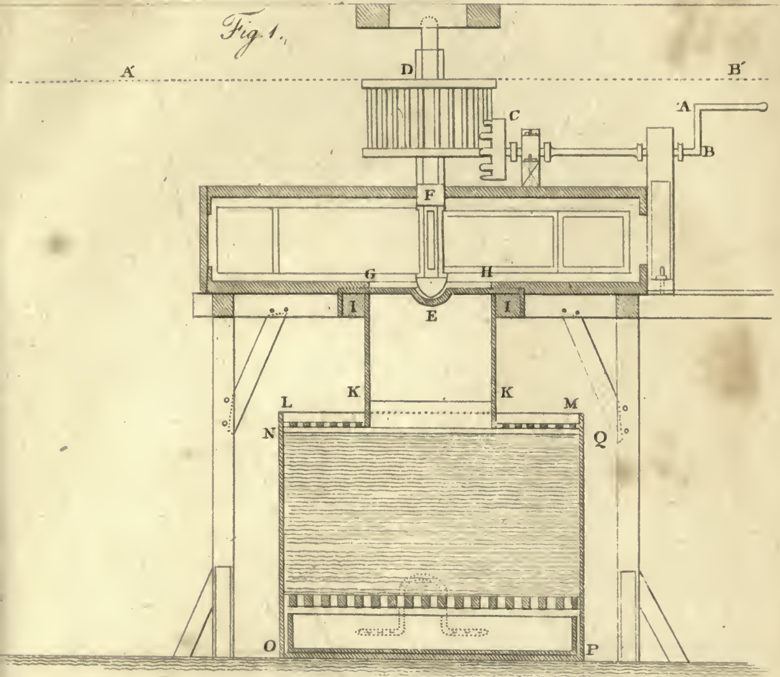

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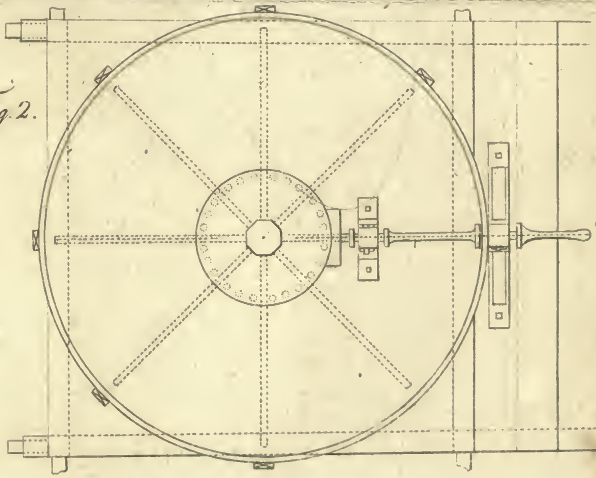

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END OF THE TWENTY-EIGHTH VOLUME.

ERRATA.

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117	18	*for* my *read* any
178	13	*for* of *read* off
256	24	*for* smaller plants *read* semishrubs
258	19	*for* colour *read* contour.
317		At Art. XI there should have been a reference to Phil. Trans. for 1810, p. 204.
330		*in the second side note for* on *read* an

Stratford, Printer, Crown Court, Temple Bar.